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SIR R. S. BALL, LL.D., F.R.S., President, in the Chair.

Henry Ellis, Little Heath, Potter's Bar;
Peter Matthews, 65 Gordon Mansions, W., and 102 Fenchurch Street, E.C.;
Captain P. B. Molesworth, R.A., Trincomali, Ceylon;
W. R. Reynolds, 61 Fairholt Road, Stamford Hill, N.; and
William Edward Sparkes, 5 Roker Terrace, Sunderland,

were balloted for and duly elected Fellows of the Society.

The following candidates were proposed for election as Fellows of the Society, the names of the proposers from personal knowledge being appended :—

Lieutenant Tristan Dannreuther, R.N., F.R.G.S., H.M.S. *Leander*, Pacific Station, Esquimault, British Columbia (proposed by S. Wellesley Johnson); and
Charles Thomas Whitmell, M.A., B.Sc., H.M. Inspector of Schools, Invermay, Headingley, Leeds (proposed by G. A. S. Atkinson).

The following were proposed by the Council as Associates of the Society :—

O. Backlund, St. Petersburg;
Edward Emerson Barnard, D.Sc., F.R.A.S., Yerkes Observatory, U.S.A.;

Sherburne Wesley Burnham, M.A., F.R.A.S., Government Building, Chicago, U.S.A. ;
Lieut.-Col. G. Defforges, Service Géographique de l'Armée, Paris ;
James Edward Keeler, D.Sc., F.R.A.S., Director of the Lick Observatory, Mount Hamilton, California, U.S.A. ;
Henry A. Rowland, Johns Hopkins University, Baltimore, Md., U.S.A. ;
Wilhelm Schur, Observatory, Göttingen, Germany.

Seventy-five presents were announced as having been received since the last meeting, including, amongst others :—

W. T. Lynn, Remarkable Eclipses, 3rd edition ; F. McClean, Comparative photographic spectra of stars to the $3\frac{1}{2}$ mag. ; and Comparison of oxygen with spectra of Helium stars and summary of spectra of southern stars, presented by the authors. Galileo, Opere, edizione nazionale, vol. vii., presented by the Italian Government ; Laplace, Œuvres complètes, tome xii., presented by the Paris Academy ; L. Weinek, Photographischer Mond-Atlas, Heft 1, 2, presented by Dr. Weinek ; Old transit instrument, formerly belonging to Dr. Longfield of Cork, presented by Miss Lecky, executrix of the late R. J. Lecky, F.R.A.S.

Publication of Eclipse Results.

The question of the publication of the reports of the observers sent out by the Joint Permanent Eclipse Committee of the Royal and Royal Astronomical Societies has been under consideration by that Committee and by the Councils of the two Societies which it represents ; and by the generosity of the Royal Society a very satisfactory solution has been arrived at. The main difficulty lay in the choice of the series of publications : if the reports were communicated, as on other occasions, to the Royal Society, it was felt that they would not be easily accessible to many astronomers ; while if, on the other hand, they were printed in the publications of this Society they would not be sufficiently brought to the notice of physicists and others. To print them in both series would have obvious disadvantages from the point of view of economy.

The solution arrived at is as follows : The Preliminary Reports are to be printed in the *Proceedings of the Royal Society*. After the usual copies have been struck off for the use of Fellows of the Royal Society, the printers will then be directed to strike off one thousand copies more, altering merely the paging to a series of figures in brackets—(1), (2), &c.—so that the reports

may appear as an Appendix to a volume of the *Monthly Notices* [the sizes of the pages in *Proc. R.S.* and *Monthly Notices* are almost the same], the extra expense of this one thousand copies being borne by this Society. Fellows of this Society may expect to receive this Appendix during the present summer for inclusion in the present volume of *Monthly Notices*.

As regards the full and final reports these will be printed as a volume of *Philosophical Transactions of the Royal Society*, and copies of this volume will be presented by the Royal Society to our Fellows who apply for them, and to such observatories, institutions, &c. as receive copies of our Memoirs.

It may be added that our Council have referred to the J.P.E. Committee the question whether it would not be desirable to invite other observers than those sent out by the Committee to communicate their results for publication along with these final reports in the volume of *Philosophical Transactions*.

H. F. Newall, }
H. H. Turner, } *Secretaries.*

Reply to Dr. Rambaut's Note "On the Effect of Chromatic Dispersion." By David Gill, C.B., F.R.S., Her Majesty's Astronomer at the Cape of Good Hope.

In the *Monthly Notices* for 1898 March Dr. Rambaut communicates a long and elaborate note on a previous paper of mine entitled "On the Effect of Chromatic Dispersion of the Atmosphere on the parallaxes of *α Centauri* and *β Orionis*."

I desire before entering on any criticism of this note to acknowledge the value of Dr. Rambaut's original paper in so far as it suggests the desirability of expressing the possible effect of chromatic dispersion on parallax researches; and indeed I have acknowledged the value of that suggestion not only in my original paper, but in the best manner in my power, viz. by adopting it. In the determinations of stellar parallax, which I have since published, I have given the effect of chromatic dispersion in terms of $\Delta\beta$ on the result, or, when the observations admitted of it being done, I have determined the special value of $\Delta\beta$.

In no case, however, has $\Delta\beta$ proved to be a sensible quantity.

The point in Dr. Rambaut's original paper, which I was compelled to dispute, was the conclusion that my results were affected by a systematic error following the law of the effect of chromatic dispersion. Had such an error existed to the extent which Dr. Rambaut's discussion professed to show (viz. $\Delta\beta = 0''\cdot095$, with factors of $\Delta\beta$ ranging from $-2\cdot3$ to $+0\cdot7$, producing systematic errors amounting to $+0''\cdot2$ and $-0''\cdot07$) I should certainly have been guilty of extreme want of circumspection in having over-

looked them ; and if such systematic errors had really existed, the gravest doubt would be thrown on the reliability of all refined determinations of solar or stellar parallax.

I believed, and I still hold, that in my original paper I conclusively proved that the corrections $x_{,}$ $x_{,,}$ $x_{,,,}$ and $x_{,,,,}$ were due to real subjective errors, and that the true value of $\Delta\beta$ derivable from the observations of *α Centauri* is quite insensible (viz. $-0''{\cdot}015$ with the probable error $\pm 0''{\cdot}059$), and that my original value of the parallax of *α Centauri* remains unchanged.

In support of his original contention, however, Dr. Rambaut proceeds to make the following statements, which I quote and designate for brevity as follows :—

(*a*) "It has always seemed to me most unsatisfactory to assume empirical corrections to remove the larger residuals and then, appealing to the smallness of the resulting residuals as a proof of the accuracy of the observations, to compute the probable error of the result by means of these reduced residuals."

(*b*) "It is a mistake in principle to assume the existence of constant errors unless one is absolutely driven to it."

(*c*) "The fact of an error of the same sign affecting a number of observations in a series does not prove it to be a constant, and in assuming it to be so we assume not only its magnitude, but also the law of its operation."

(*d*) "If we assume the existence of constant but unknown errors, and apply corrections for them, we must not ignore the fact that they are *errors*, and we must therefore include them in computing the probable error of an observation."

(*e*) "The irregular errors of observation are just as *real* as the systematic errors, and until we are assured that the latter are really constant, or until we have established the law of their operation, we must treat them as being subject to the law of frequency of errors, and take them into account in estimating the probable error of the result."

Now all these statements excepting (*b*) are quite incontrovertible. I fully grant their truth, and only contend that neither in my original discussion of the parallax of *α Centauri* (*Mem. R.A.S.* vol. xlviii.) nor in my subsequent one (*Monthly Notices*, vol. lviii. p. 63) have I omitted to regard them.

With regard to (*b*), however, I would prefer to render it as follows :—

"In every refined investigation in which the conditions of observation are in any way systematically changed the astronomer must take the greatest care to ascertain whether such systematic change in the conditions of observation is accompanied by a corresponding change in the results, and should so arrange

his observations as to afford the data necessary for a complete discussion of the law of such systematic error and for the computation or elimation of its effects."

I think there are very few experienced astronomers who will deny the soundness of this statement, or who will not agree that the danger of neglecting such precaution is likely to lead to erroneous conclusions.

In replying to Dr. Rambaut's criticism I shall therefore confine myself to two points.

1st. That my observations were affected by true systematic errors, and that these errors were traced to their true source, viz.—to four *true* constant subjective errors due to the four different conditions of measurement.

2nd. That the method employed to compute the necessary corrections and the probable error of the resulting parallax was the only accurate one.

First, then, what are the conditions which distinguish true systematic errors from those which are accidental, or which distinguish between legitimate and empirical corrections?

Primarily, of course, it is necessary to regard any physical source of possible error (such, for example, as Dr. Rambaut's $\Delta\beta$ with the factor $\tan\zeta\cos(p-q)$) as a legitimate unknown quantity which may properly be introduced into the equations. But besides such obvious possible sources of error, there may be others which are personal or subjective, *provided that the conditions of observation are changed.*

It is, of course, open for Dr. Rambaut to deny that this is the case. In his statement (*b*) he expresses his unwillingness to assume the existence of constant errors unless absolutely driven to it. This seems to me a very strange statement for any experienced astronomer to make. In my opinion it rather becomes the astronomer who aspires to obtain truthful results to be continually on the outlook for sources of possible systematic error.

If Dr. Rambaut had any experience of refined heliometer observations it would perhaps be easier to convince him. If, for example, he had made heliometer observations, without the aid of a reversing prism, of the angle between a distant star (50′ or 60′) and one of the components of a fairly close double star at very various hour-angles, and on opposite sides of the meridian, he would instinctively feel that, in pointing on such an unsymmetrical object, he could not be certain that his observations were unaffected by personality depending on the hour-angle or on the direction of measurement with respect to the observer's eyes.

As a proof of the general existence of such an instinctive feeling, and of the necessity for optical symmetry and constancy in apparent direction of measurement in his work, every careful

modern heliometer observer has now adopted the use of the reversing prism which I first introduced in my heliometer observations of *Mars* in 1877.

Had the reversing prism been employed in the observations of *α Centauri* in question much labour would have been saved, because the necessity for measures to determine the systematic corrections dependent on the direction of measurement with respect to the eyes of the observer would have been avoided.* But I was careful to make the special observations necessary to determine these systematic errors, discussed them, and believed that astronomers generally had adopted my conclusions as sound. Dr. Rambaut has denounced these corrections as *empiric*, and comes to the conclusion that my so-called empiric corrections can in reality be traced to a true physical origin, viz. chromatic dispersion of the atmosphere. This is at least his original contention, which provoked my criticism, and he has not yet admitted his error. Let me show once more, and if possible more forcibly, that Dr. Rambaut entirely fails to prove his thesis. Dr. Rambaut says "that Dr. Gill's argument founded on a comparison of the means of the residuals in each group is of *absolutely no value whatever*, his solution being artificially constructed to fulfil the condition that these means should each be zero." The latter part of this statement is perfectly true; but I have never employed any such argument, and it hardly required nearly two pages of algebra to show the self-evident fact that my small mean residuals of the groups in the fourth place of decimals only go to demonstrate the general arithmetical accuracy of work which was computed to three decimal places only.

But in examining the accuracy of Dr. Rambaut's solution it was surely reasonable and fair that I should divide the residuals of his solution into four groups corresponding to the four circumstances of measurement. If with the introduction of his $\Delta\beta$ in place of my $x_{,}\, x_{,,}\, x_{,,,}\, x_{,,,,}$ Dr. Rambaut had succeeded in representing all these groups of equations within reasonable limits of error, then he would have triumphantly proved his point, viz. shown that my $x_{,}\, x_{,,}\, x_{,,,}$ and $x_{,,,,}$ were unjustifiable empirical corrections, and that his value of $\Delta\beta$ was a true quantity. Unfortunately for Dr. Rambaut's argument, such is not the case.

Take the group 3, the largest group, containing 41 equations: *all* his residuals in this group, with three very small exceptions (viz. $+0^{R}\cdot007$, $+0^{R}\cdot007$, and $+0^{R}\cdot005$), are negative, the mean value being $-0^{R}\cdot0139$.

Now Dr. Rambaut finds, from his own solution, that the probable error of a single observation is $\pm0^{R}\cdot0131$; in other words, *the mean residual in this group of* 41 *observations is greater than*

* The reasons which compelled me very reluctantly to abandon the use of the reversing prism are given (*Mem. R.A.S.* vol. xlviii. p. 11), and need not be here repeated.

the probable error of a single observation ! ! If this fact alone does not prove the existence of a systematic error, and the neglect of a true systematic correction of some kind in Dr. Rambaut's solution, I think it will be difficult indeed to convince him ! The chance that the mean residual of a group of 41 observations, unaffected by systematic error, shall exceed the probable error of one observation is about the same as that in tossing a coin "heads" will come up 41 successive times or "tails" 41 successive times.

Has Dr. Rambaut considered what are the chances of tossing 41 successive "heads" ? If so, he will have determined the chances in favour of the truth of this part of his solution.

In group 4, which consists of 13 observations, Dr. Rambaut's mean residual is $+0^{R}\cdot0246$, so that the mean value of the residuals of 13 observations is nearly double the probable error of a single observation ! ! Further comment is unnecessary.

Turning now to my own solution. Dr. Rambaut questions "the validity of Dr. Gill's method of introducing constant corrections to smooth down the roughness of the observations, unless he takes into account the hypothetical character of these corrections in estimating the probable error of the result."

Here, again, Dr. Rambaut is in principle perfectly correct, but absolutely wrong as to his description of the reasons which led me to adopt the corrections in question. Before making such a statement, he should have carefully considered the circumstances which led me to the conclusion that the corrections in question were true subjective constant errors, due to the unsymmetrical character of one of the objects under observation. These circumstances are all fully detailed in my original memoir. First of all, I was fully aware of the possible effect of change of conditions of measurement, because, as early in the series as it was possible to do so, I began to make observations in the early morning as well as in the evening, so as to determine whether any such systematic differences existed. The immediate reduction of the observations showed that such errors really existed, and there was even then strong evidence that these errors were very closely dependent on the direction of measurement with respect to the line joining the eyes of the observer, so that careful note was made of this direction in the subsequent measures (*loc. cit.* p. 17). But I was careful not to assume this to be the case. In his criticism of the "hypothetical character of these corrections" Dr. Rambaut entirely omits to note the great care which I took to ascertain whether these errors were really constants or functions of the hour-angle, viz. by making and discussing my Solution III., in which, instead of assuming the corrections to be constants, they are expressed in the form—

$$x + \cos(p-q)\,\alpha + \sin(p-q)\,\beta + \cos 2(p-q)\,\gamma + \sin 2(p-q)\,\delta$$

(*loc. cit.* p. 35 *et seq.*).

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This solution led to a value of the parallax practically identical with that of Solution I., but with a rather larger probable error for the single observation, tending to show that the errors are more nearly constants than functions of the hour-angle. This conclusion is, however, very much strengthened if we arrange the residuals of Solution III. in order of hour-angle in the same manner as those of Solution I. are given in my paper, *Monthly Notices*, vol. lviii. pp. 59–62. Then we find that in group 1 the residuals begin at $8^h\ 17^m$ S.T. with a negative tendency, but become all positive towards the end of the first group; then in the immediately following group 2 the residuals suddenly change, and are chiefly negative in sign. What is the cause of this sudden change? Why, assuredly the change depending on the change of direction of measurement with relation to the line joining the eyes of the observer—there is nothing else to account for it. Passing from group 2 to group 3 there again occurs a sudden change in the sign of the residuals from negative to positive, showing that *no correction which is a function of the hour-angle will satisfy the observations*! Again in group 3 the residuals in the early part of the group are all positive, and nearly all negative towards the end, and then in the immediately following group 4 there is a sudden change towards the positive residual.

On the other hand if we take my Solution I., in which the corrections are assumed to be constants, we find that the positive and negative residuals are most fairly distributed throughout each group, notwithstanding the large range of hour-angle in each.*

If these are not valid proofs that the necessary corrections are true constants then I am unable to understand what valid proofs are.

These proofs contrast very favourably with the extraordinary solution originally proposed by Dr. Rambaut in which in a group of 41 successive equations (the order being that of the magnitude of the factor of a term the existence of which he is seeking to prove) 38 of the residuals have the same sign, and the mean residual exceeds the probable error of a single observation; or in another group of 13 observations, in which his mean residual is double his probable error for a single observation.

Again if we take group 3 of my Solution I. we find that the factors of $\Delta\beta$ vary from $+0\cdot18$ at the beginning to $-0\cdot98$ at the end of the group; but there is no evidence amongst my 41 residuals of any systematic tendency to increase or diminish from one end of the group to the other, as must necessarily be the case if $\Delta\beta$ were a sensible quantity.

Similarly, in group 4 the factors of $\Delta\beta$ vary from $-1\cdot08$ to $-2\cdot39$; and here again we find in the run of my residuals no

* If Dr. Rambaut should reply to this paper I trust he will avoid the mistake he previously made, and note that this argument refers to the run of the residuals in each group with reference to hour-angle in the group itself, and not to the agreement of the mean residuals of the different groups.

indication in the character of the residuals which points to a real value of $\Delta\beta$.

Besides all this I have given the more general solution (*Monthly Notices*, vol. lviii. p. 63), in which I have introduced Dr. Rambaut's $\Delta\beta$ as an unknown quantity, and derived its value, viz. $-0''{\cdot}015$ with the probable error $\pm 0''{\cdot}059$. It has therefore no proved reality, and its most probable value has the opposite sign to that found for it by Dr. Rambaut.

I think it is hardly worth while to discuss irrelevant abstract statements such as "if Dr. Gill will allow me eight such corrections I can reduce the residuals so that none of them will exceed $0^{R}{\cdot}010$; twelve such corrections will reduce them all below $0^{R}{\cdot}005$, &c." . . .

I must point out to Dr. Rambaut that in the discussion of astronomical data common sense and a reasonable use of the critical faculty are absolutely necessary. If there were twelve *a priori* common-sense reasons for the introduction of 12 constants in a discussion of an investigation of stellar parallax it might be desirable to introduce the whole of them. But, of course, such corrections must not be assumed without some valid reason, such as a change in the method of observation or some instrumental change, or change in some external physical condition; and it would be necessary to determine by careful discussion—such as I have given in the case of *α Centauri*—whether the corrections are real and are truly constant or demonstrably follow some other law. Dr. Rambaut would also find that the introduction of 12 unknown constants would so reduce the weight of his resulting parallax as to make it quite indeterminate.

It is true that Dr. Rambaut states (page 259), "I do not for a moment mean to imply that a solution of this sort" (the introduction of arbitrary corrections)* "is comparable with Dr. Gill's, where his groups were arranged for him by considerations as to the position of the observer's head with regard to the line joining the stars. I merely intend to point out how misleading an appeal to the smallness of the residuals is in such a solution." I have nowhere stated that the smallness of the residuals should alone be appealed to in such a case; on the contrary, in the illustration which I gave from the discussion of my observations of *β Orionis* I have only introduced the unknown quantities $\Delta\beta$ and π, the existence of which were physically possible, and the introduction of either or both of these quantities *reduced* the sum of the squares of the residuals, but has *augmented the square of the mean error of a single observation.*

On these grounds I held that the values of $\Delta\beta$ and π were most probably insensible.

It is true that I have pointed out (page 57) that "it is a suspicious fact that the weight of an observation by Dr. Rambaut's solution is reduced to less than one half of that from my Solution I."

* The words in parenthesis are mine, and inserted for explanation.

Dr. Rambaut tries to force this statement into an expression that my chief grounds of objection to his solution are that the residuals are increased ; whereas the real and solid ground of my rejection of his result is based on the fact that there is no correspondence whatever between his residuals and the coefficients of the term which he has introduced for the purpose of bringing the observations into systematic accord.

How does he account for the *sudden* systematic change of sign in his residuals from one group of observations to the next? That is the real question ; that is the point which he studiously avoids, and endeavours to cover by a cloud of formulæ relating to a side issue.

These are my arguments, to which I am most anxious that Dr. Rambaut should reply ; and I ask him to state, after he has examined them, whether he still finds that his $\Delta\beta$ has a real value approaching the probable error of a single observation. This was his original contention. If Dr. Rambaut had proved his point it would have thrown the gravest doubts on all previous investigations of stellar and planetary parallax ; as it is, I find, from every point of view, that his solution is absolutely worthless, and entirely fails to represent the observations, and that the more closely the details and results of my own discussion are examined, the more fully do they prove the truth of my original solution.

Before entering on the entirely new contention which Dr. Rambaut has dragged into the discussion, viz. that my computation of the probable error of my result is erroneous, it seems necessary to clear the ground somewhat.

The general equation under consideration is of the form

$$x + by + cz = n,$$

where y is a term depending on the time, and z a term depending on the parallax.

The point where Dr. Rambaut is in error is as to the true meaning of x in his discussion. The term x is apparently interpreted by Dr. Rambaut to mean "the true correction to the assumed difference between the two measured distances α and β freed from parallax at the adopted epoch."

Therefore Dr. Rambaut says, if I follow him rightly, that if the observations are of four different kinds, or made under four different conditions of observation, each of which is affected by a constant error, then the equations will divide themselves into four groups of the following types :—

Type	I.	$x + a_1 + b_1y + c_1z = n_1$
,,	II.	$x + a_2 + b_2y + c_2z = n_2$
,,	III.	$x + a_3 + b_3y + c_3z = n_3$
,,	IV.	$x + a_4 + b_4y + c_4z = n_4$

and he proceeds very elaborately to point out how the true weights and most probable values of x, a_1, a_2, a_3, a_4, y, and z should be determined.

The initial error which Dr. Rambaut has made, and which vitiates the whole of his subsequent conclusions, is his assumption that x is a true determinable constant independent of a_1, a_2, a_3, and a_4. This would only be true if we could *a priori* assume some relation between the values of a_1, a_2, a_3, and a_4, or that $a_1+a_2+a_3+a_4$=a known quantity. But as no such relation is known, it is only possible to determine from these equations $(x+a_1)$, $(x+a_2)$, $(x+a_3)$, $(x+a_4)$, y and z; for which I have written for simplicity $x_,$, $x_{,,}$, $x_{,,,}$, $x_{,,,,}$, y and z.

In other words our four groups of observations supposed to be affected by four constant but unknown sources of systematic error cannot yield a true value of x (in the sense in which I have defined it), because the mean of all our observations (apart from terms in y and z), or the means of our four derived values of the systematic errors, cannot be assumed to be free from systematic error. Our problem fortunately does not require a determination of x, but merely of $x_,$, $x_{,,}$, $x_{,,,}$, and $x_{,,,,}$, as I have defined them. I have already, in the earlier part of this paper, shown *that within the limits of the small accidental errors of observation, the systematic errors of the four groups cannot be represented by any continuous function of the hour-angle*; they are therefore true constants, and therefore also $x_,$, $x_{,,}$, $x_{,,,}$, and $x_{,,,,}$ are true constants. Being true constants there is but one legitimate method of determining $x_,$, $x_{,,}$, $x_{,,,}$, and $x_{,,,,}$, viz. to treat them as true unknown quantities, and to determine their values, weights, and probable errors by the usual Gaussian method of least squares simultaneously with the values, weights, and probable errors of y and z.

The results are given on page 28 of my original memoir.

I am quite at a loss to understand such statements as the following in Dr. Rambaut's paper:—"But in any case if we assume the existence of constant but unknown errors, and apply corrections for them, we must not ignore the fact that they are *errors*, and we must therefore include them in computing the probable error of an observation. This Dr. Gill has not done, and the computed probable error of his result is consequently very much underestimated. In his recent paper Dr. Gill argues with regard to these corrections that they represent real quantities, with the implication that if this is the case they need not be taken into account in calculating the probable error."

Every competent person will admit the truth of the first part of this statement, but I should be glad to be informed where I have implied that in computing the probable error of the parallax or of the unknown corrections the errors of the determinations of these corrections should not be taken into account. My original normal equations (omitting the term depending on the square of the time) were:—

$24{\cdot}50x_{,}$					$+\ 8{\cdot}04y$	$+\ 6{\cdot}86y'$	$+15{\cdot}99z$	$=+2{\cdot}16$
	$+13{\cdot}00x_{,,}$				$+\ 7{\cdot}99$	$+\ 8{\cdot}35$	$+\ 9{\cdot}32$	$=+0{\cdot}99$
		$+37{\cdot}00x_{,,,}$			$+12{\cdot}61$	$+18{\cdot}82$	$-\ 8{\cdot}69$	$=-1{\cdot}78$
			$+12{\cdot}00x_{,,,,}$		$+\ 2{\cdot}71$	$+\ 2{\cdot}53$	$-\ 6{\cdot}64$	$=-0{\cdot}62$
$8{\cdot}04$	$+\ 7{\cdot}99$	$+12{\cdot}61$	$+\ 2{\cdot}71$		$+36{\cdot}56$	$+37{\cdot}32$	$+14{\cdot}95$	$=+1{\cdot}42$
$6{\cdot}86$	$+\ 8{\cdot}35$	$+18{\cdot}82$	$+\ 2{\cdot}53$		$+37{\cdot}52$	$+47{\cdot}59$	$+11{\cdot}11$	$=+0{\cdot}84$
$15{\cdot}99$	$+\ 9{\cdot}32$	$-\ 8{\cdot}69$	$-\ 6{\cdot}64$		$+14{\cdot}95$	$+11{\cdot}11$	$+48{\cdot}42$	$=+5{\cdot}65$

Now the coefficient of z (the term depending on the parallax) in the normal for z of these equations is 48·42; and if we neglect the errors of the determination of $x_{,}$, $x_{,,}$, $x_{,,,}$, $x_{,,,,}$, and y, the weight of z would necessarily be 48·42. If, in computing the weight of z, we have regard to the error of x and y, but disregard the effect of the errors of determination of the systematic corrections—in other words, if we add all the normals in $x_{,}$, $x_{,,}$, $x_{,,,}$, and $x_{,,,,}$, and treat the normal so formed as an equation in x, we obtain z with the weight 42.

But if we eliminate z with its weight from the whole of the six normal equations, we find the weight of z to be diminished from 42 to 22, in consequence of the effect of the errors of the determination of the other unknown quantities ; and this is, of course, the true weight of z.

Also in computing the square of the mean error, instead of dividing the sum of the squares of the residuals by the number of equations, the sum is divided by the number of equations *minus* the number of unknowns. So far as I am aware, there is no other legitimate method of proceeding. The method is too well known to require further discussion or justification.

In conclusion, I may briefly criticise the five solutions, the results of which are summarised by Dr. Rambaut on page 279.

(1) Solving without regard to the systematic differences of the four different kinds of observation.

$$\pi = 0''{\cdot}819 \pm 0''{\cdot}014.$$

Dr. Rambaut admits that the residuals show this solution to be inadmissible, and the deduced parallax is much too large. This is pointed out on page 23 of my original memoir. On what ground is Dr. Rambaut's original solution then to be accepted, where the residuals are just as large and as systematically discordant ? The comparatively small probable error of the result is due to the large algebraic weight of z.

This solution is a good illustration of the errors into which an astronomer would fall who persistently refuses to "assume the existence of constant errors unless absolutely driven to it."

(2) and (3) Dr. Rambaut's method of solving, by which

$$\pi = 0''{\cdot}769 \pm 0''{\cdot}014 \text{ and } \pi = 0''{\cdot}780 \pm 0''{\cdot}018.$$

The initial error of this solution is, as already shown, Dr. Rambaut's erroneous assumption that x is a real quantity—which assumption forces him to solve the equation in four different groups. He appears to overlook the fact that he cannot derive the most probable values of $x_{,}$, $x_{,,}$, $x_{,,,}$, and $x_{,,,,}$ by this process from a first approximation, because their resulting values depend on values of y and z which are not the definitive values of these quantities. To obtain a nearer approximation to the truth he would require to make a second approximation, employing in it the values of y and z derived from their four values combined with regard to their weights. Introducing these more approximate values of y and z, having regard to their probable errors as affecting the weights, he could make a second solution, and so on for a third and fourth approximation; whence he would finally arrive at the same most probable values of the unknowns and their true weights and probable errors, which I found by the more direct process.

(4) Dr. Rambaut's original solution, introducing $\Delta\beta$ and excluding personal errors,

$$\pi = 0''{\cdot}780 \pm 0''{\cdot}018.$$

This solution does not represent the observations any better than solution (1), which Dr. Rambaut himself condemns. The probable error of the single observation is just as great as in solution (1), and the systematic errors of the groups just as large, indeed larger.

This is the solution in which the mean residual of forty-one successive observations (arranged in order of hour-angle) is greater than Dr. Rambaut's own computed probable error of a single observation.

(5) My own solution

$$\pi = 0''{\cdot}747 \pm 0''{\cdot}013$$

alone is rational and complete.

The main point of my original paper, and of this one also, however, is that the true value of Dr. Rambaut's $\Delta\beta$ is entirely insensible, and therefore that chromatic dispersion has quite an insensible effect on my derived parallax of α *Centauri*.

Royal Observatory, Cape of Good Hope:
1898 *May*.

Note concerning Diffraction Phenomena, &c.
By H. F. Newall, M.A.

I regret that my effort to make my note (*Monthly Notices*, lviii. [present volume], p. 3) as short as possible has led Mr. Wadsworth to think that I have done injustice to his work by the appearance of wholesale criticism. I had hoped to guard against this appearance by the explicit reference which I made to "results obtained with large and small reflectors and refractors"; and I cannot think that my intention was generally misunderstood. I am only sorry that a great press of business before leaving for India last winter made me forget my original intention of sending my note in manuscript to Mr. Wadsworth.

I am unwillingly led to believe that silence on my part now would be misunderstood. But I have little or nothing to add to what I have already said, except perhaps that it would not occur to me to interpret the paragraphs in the *Encyclopædia Britannica* in the way that Mr. Wadsworth has done. The lines quoted by him cannot be regarded as the *conclusion* of a line of argument, as Mr. Wadsworth represents them to be (*Astrophysical Journal*, vii. p. 79); on the contrary they form the introduction to a very elegant treatment of a special example of illumination near the border of an image; and, though they contain statements which are not rigorously consistent, the meaning is beyond doubt clear. Lord Rayleigh is there dealing with a case in which the *scale* of the diffraction pattern does not come into consideration, and consequently there is no need to consider the focal length in the expression for the total illumination.

On the Actinic Qualities of Light as Affected by Different Conditions of Atmosphere. By the Rev. J. M. Bacon.

Apart from its relation to photography, this inquiry is of importance to the astronomer as involving considerations with regard to definition, the choice of observing stations, &c.

I have been led step by step to the conclusions at which I have arrived by the following series of tests often and variously repeated :—

Using in all cases uniform samples of sensitised paper or films, and as far as possible eliminating all accidental sources of error, I have, by means of subdued and prolonged exposures, made comparisons between the action of light proceeding from grey or blue sky.

(1) After traversing a length of tube admitting no extraneous light and containing only air at the surrounding temperature.

(2) After traversing the same tube containing various admixtures of smoke and floating mote particles of different kinds.

Here, as was expected, the action of the light was impeded in proportion to the presence of foreign matter.

Similar experiments being repeated with the same media, but diffused light being at the same time freely admitted on all sides, it was found easy so to arrange matters that a slight addition of white smoke rendered the action of the light more rapid, doubtless owing to myriad reflections from the surfaces of its particles

Next, experiments similar to the above were made with air at normal temperature, which was compared with air variously heated. Here currents, condensation, &c., being as far as possible avoided, it was generally found that heated air retarded photographic action until the sensitive surface itself became materially heated, when action might be considerably accelerated.

Yet more marked comparisons were obtained between the effect of ordinary air and the air in which spirit was evaporating. In these cases as the air became charged and its temperature lowered by the evaporation, action was always accelerated in a striking degree.

This is but our familiar experience. In moist weather the distance is blue, clear, and sharp. In warm dry weather distance becomes grey and fades out. In an east wind all things are grey.

Those accustomed to out-door photography are aware how overpoweringly dazzling their sitters will often find the daylight after rain, even though the sky be overcast. Thus in moist weather the atmosphere would seem to be not only more transparent but to convey more diffused light, and it does not suffice as an explanation that floating foreign matter has been cleared away.

In India, at the time of the late eclipse, rain having been absent for a long period, it was found that the action of even the brightest sun on a photographic plate was far less rapid than might have been expected, and became markedly more energetic subsequently when rain clouds overcast the sky. This I exemplified by various photographic views which were projected on the screen at the last meeting of the Society.

At the same meeting I exhibited a series of photographic landscape pictures taken at regular intervals and with uniform exposure while the eclipse was in progress, which pictures went to show that the slow shutting off of the light, commonly noticed before totality, and its rapid return after totality was an actual fact, and not a mere subjective phenomenon. With reference to this Professor Turner has suggested that the explanation may lie in the sudden condensation of moisture occasioned by fall of temperature. The moisture thus condensed had clearly existed previously in the air only in finer subdivision, and its exalted power of transmitting light would seem due to its coalescing in larger particles. It is quite otherwise condensed when the "water dust" produces only the veiling of mist.

Have we some sort of analogy here in the passage of light through grains of, say, broken glass or crystal? The more minute

the grains, *i.e.* the more molecular continuity is destroyed, the more is the light scattered and lost. May it not be so also with regard to moisture particles in suspension? The more subdivided they are, the greater number of limiting surfaces are there to scatter and impair the light.

But I am led to conceive that the intensity of light reaching us from the sky is occasionally influenced by another cause. Arago and others have pointed out, not only the familiar fact that clouds may be luminous, but that certain fogs may be phosphorescent, and they suggest that some such cause may account for certain abnormally light moonless nights; but I would go further and suggest that even clear sky may be phosphorescent also. We are familiar with various bodies often microscopic, in life or death, fitfully phosphorescent. Various noctilucæ render the ocean at times luminous, and light up the actual horizon. Are there no microcosmic organisms in higher strata of the ocean above us which behave similarly?

Again, phosphorescence of such organic matter as may well be in suspension in the atmosphere is thought to be connected with the presence of ozone, which in the atmosphere is a varying quantity. And, again, if it be actually true that it is often "darkest before dawn," then it is conceivable that there may be floating particles of the nature of phosphorescent sulphides or luminous pigments which lose their luminescence as the night proceeds.

A series of experimental photographic records seem to indicate that varying tracts of the night sky are occasionally to some extent self-luminous, but these results need further confirmation on clear moonless nights.

On some Attempts to Counteract by Instrumental Adjustments certain Effects of Refraction in Stellar Photography. By Arthur R. Hinks, B.A.

(*Communicated by Sir R. Ball.*)

Attention has recently been called to the possibility of counteracting to a large extent by instrumental adjustments the effects of refraction in displacing by a continually varying amount the field to be photographed. Dr. Rambaut has shown (*Monthly Notices*, 1896, vol. lvii. p. 50) that the variation with the hour-angle of the component in R.A. of the refraction can be met by suitable changes in the rate of the driving clock. When the polar axis of the telescope is adjusted to the true pole, the requisite change of rate for stars of small north polar distance becomes very large. Mr. Davidson has shown (*Monthly Notices*, 1897, vol. lviii. p. 4) that this difficulty can be overcome

by adjusting the polar axis to the apparent pole as affected by refraction.

Neither Dr. Rambaut nor Mr. Davidson has given numerical details of the variation of the component in declination of the refraction. Yet in practice this component is the more important of the two, while the instrumental means of correcting for it are comparatively inadequate. The device adopted by Dr. Rambaut of introducing a change in clock rate, and consequent continuous trail in R.A., does away almost entirely with the need for small discontinuous hand corrections. But it does not appear that there is any instrumental device in use which will give a corresponding continuous trail in declination, and do for the following in this coordinate what the change in clock rate does for the R.A.'s.

I have been led, therefore, to examine the effect of a displacement of the polar axis of a telescope on the following in declination in the hope of finding a means of perfecting this, even at the expense of the following in R.A. The effect on the latter can then be compensated by modifying the alteration in the rate of the driving clock proposed by Dr. Rambaut.

I. *Effect on the following in declination of a displacement of the instrumental pole.*—Let the elevation of the instrumental above the true pole be e, and let the instrument be set for declination Δ. Let h be the hour-angle of the point to which the telescope is directed at any moment. It is easily seen that the declination δ of this point $= \Delta - e \cos h$, so long as e is small, and δ is not very nearly 90°.

$$\therefore \frac{d\delta}{dh} = e \sin h.$$

The projection on the sky of the intersection of the cross wires of the guiding telescope will therefore be moving in declination at the rate $e \sin h$. The values of this expression in seconds of arc per hour for various values of e and for every hour of hour-angle are given in the following table. The elevation 45″·3 is the adjustment proposed by Mr. Davidson to the apparent pole as affected by refraction in the latitude of Cambridge.

TABLE I.

Rates of Motion in Declination per hour, due to Elevation of the Instrumental Pole.

Elevation.	Hour-angle West. 0h.	1h.	2h.	3h.	4h.	5h.
30″	0·0	+ 2·0	+ 3·9	+ 5·6	+ 6·8	+ 7·6
45·3	0·0	3·1	5·9	8·4	10·3	11·5
60	0·0	4·1	7·9	11·1	13·6	15·2
80	0·0	5·4	10·5	14·8	18·1	20·2
100	0·0	6·8	13·1	18·5	22·7	25·3
120	0·0	8·1	15·7	22·2	27·2	30·4

and so on.

The displacement in declination due to refraction is $\Delta\delta = k \tan z \cos q$, where q is the parallactic angle. And if we put $\tan\theta = \tan c \cos h$, as in Mr. Davidson's paper, this becomes after some reduction

$$\Delta\delta = k \cot(\delta + \theta)$$

$$\therefore \frac{d}{dh}\Delta\delta = k \tan c \sin h \operatorname{cosec}^2(\delta+\theta) \cos^2\theta$$

The values of this expression are given in the following table:—

TABLE II.

Rates of Motion in Declination per hour due to Refraction.

Decl.	Hour-angles. 0h.	1h.	2h.	3h.	4h.	5h.	6h.
°	″	″	″	″	″	″	″
0	0·0	+5·5	+13·2	+27·9	...	...	...
+10	0·0	3·8	8·5	16·5	+33·3	...	...
+20	0·0	2·8	6·3	11·4	20·6	+40·7	...
+30	0·0	2·3	5·1	8·8	14·7	25·2	+47·4
+40	0·0	2·1	4·4	7·4	11·6	18·0	28·7
+50	0·0	2·0	4·2	6·7	10·0	14·3	20·2
+60	0·0	2·0	4·1	6·5	9·1	12·3	16·2
+70	0·0	2·2	4·3	6·6	8·9	11·3	13·7
+80	0·0	2·5	4·9	7·2	9·3	11·0	12·2

We have now from Table I. the motion in declination of the projection on the sky of the cross wires, due to the elevation of the instrumental pole; and from Table II. we have the motion in declination of the guiding star, due to refraction. If these two quantities can be made equal the following in declination will be perfect.

It is convenient to plot the quantities in these tables as curves. If a tracing of one set is then laid over the other, it immediately appears, as is indeed plain from a comparison of the tables, that for polar stars, and for stars as far south as 20° N. decl. at small hour-angles, the effects of Mr. Davidson's proposed adjustment to the apparent pole are as valuable with respect to the following in declination as he has himself shown them to be in R.A. The outstanding drift in declination of the guiding star, which remains to be corrected by hand, is in general less than a second of arc per hour. The compensation fails, however, for equatorial stars and for stars of medium north declination at considerable hour-angles. In these cases some further adjustment is desirable.

Suppose for the moment that the instrumental pole could be adjusted with ease to any desired polar distance and hour-angle. It is clear that the effect of the adjustment to any hour-angle H would be to alter the rates given in Table I. for a

star of hour-angle h to the corresponding rates for hour-angle h—H. To examine the effect of such an adjustment it is sufficient to slide the tracing of the curves from Table I. over the curves from Table II. by an amount corresponding to the value of H.

In this way it is possible to select for a star of given declination, which is to be photographed at a given hour-angle, a suitable displacement of the instrumental pole, such that over a considerable space the portions of the curves showing the variations in declination due to refraction and displacement of the instrumental pole respectively fit one another almost exactly ; for half an hour or more the following in declination will then be almost perfect.

The following table shows for various declinations and hour-angles suitable displacements of the instrumental pole approximately determined by this method.

TABLE III.

Values of e *and* H, *Displacements of the Instrumental Pole.*

Decl.	Hour-angle. 0h.	1h.	2h.	3h.	4h.	5h.
0°	80″	100″	160″	320″	...	...
	0h	0h·2	0h·7	1h·7	...	...
+10	60″	60″	80″	160″	400″	...
	0h	0h·1	0h·3	1h·4	2h·9	...
+20	45″	45″	70″	100″	180″	...
	0h	0h·1	0h·6	1h·3	2h·2	...
+30	40″	40″	45″	80″	120″	240″
	0h	0h	0h·4	1h·3	2h·1	3h·4
+40	30″	30″	40″	60″	90″	140″
	0h	0h	0h·5	1h·2	2h·0	3h·0
+50	30″	30″	40″	50″	70″	100″
	0h	0h	0h·4	1h·0	1h·8	2h·8

Unfortunately the interest of this table is almost entirely theoretical. It is not possible in existing forms of instruments to alter the adjustment of the polar axis to suit each particular exposure, and there would be very considerable if not quite hopeless difficulties in the way of mounting an instrument with this facility of adjustment. Moreover, if the instrumental is much displaced from the true pole a very serious rotation of the field is produced, and the following would not be perfect except in the centre of the field. This point is considered in detail later.

For general photographic work, at all sorts of declinations,

and, in the case of parallax work, at considerable distances from the meridian, a complete adjustment appears to be impossible. But for work on a zone of small breadth, such as astrographic chart work, where the photographs can all be taken near the meridian, the case is different. In the latitudes of English observatories the adjustment to the apparent pole answers fairly well, except for stars of large N.P.D. ; but the adjustments given in the first column of Table III. will, I believe, be found even more satisfactory.

In low latitudes adjustment to the apparent pole is altogether wrong, except for work in the immediate neighbourhood of the pole. As an illustration of this, I give a table showing for the various observatories participating in the work of the astrographic chart, the elevation of the instrumental pole best suited to each case. The last column gives for comparison the elevation to the apparent pole.

	Latitude.	Zones (Decl.)	Suggested Elevation of Pole.	Elevation to Apparent Pole.
	° ′	° to °	″	″
Greenwich	+51 29	+90 to +65	35	46
Rome	+41 54	+64 „ +55	30	66
Catania	+37 30	+54 „ +47	30	76
Helsingfors	+60 9	+46 „ +40	30	33
Potsdam	+52 23	+39 „ +32	30	45
Oxford	+51 46	+31 „ +25	35	46
Paris	+48 50	+24 „ +18	40	51
Bordeaux	+44 50	+17 „ +11	40	58
Toulouse	+43 37	+10 „ + 5	50	61
Algiers	+36 48	+ 4 „ − 2	45	78
San Fernando	+36 28	− 3 „ − 9	55	79
Tacubaya	+19 24	−10 „ −16	30	166
Santiago	−33 27	−17 „ −23	30	92
La Plata	−34 35	−24 „ −31	35	85
Rio de Janeiro	−22 54	−32 „ −40	20	138
Cape of Good Hope	−33 56	−41 „ −51	30	87
Sydney	−33 52	−52 „ −64	35	87
Melbourne	−37 50	−65 „ −90	50	75

The calculations were made for the middle of each zone.

II. *Corrections to the rate of the driving clock.*—Any elevation of the instrumental pole will necessitate corrections to Dr. Rambaut's clock rates, similar to those proposed by Mr. Davidson. The expression for the correction is

$$+ e \cos h \tan \delta.$$

The following table gives the value of this correction when $e=100''$. The corrections for the elevation actually chosen can thence be easily calculated.

TABLE IV.

Corrections to be applied to the daily Clock Rates proposed by Dr. Rambaut for an Elevation of the Instrumental Pole of $100''$.

	Hour-angle.				
Decl.	0^h.	1^h.	2^h.	3^h.	4^h.
0°	$+0^s{\cdot}0$	$+0^s{\cdot}0$	$+0^s{\cdot}0$	$+0^s{\cdot}0$	$+0^s{\cdot}0$
+10	7·4	7·1	6·4	5·2	3·7
+20	15·2	14·7	13·2	10·8	7·6
+30	24·2	23·4	20·9	17·1	12·1
+40	35·2	33·9	30·4	24·8	17·6
+50	49·9	48·2	43·2	35·3	25·0
+60	72·6	70·1	62·8	51·3	36·3

This table is independent of the latitude, and can be used for any observatory. Dr. Rambaut's table is, of course, strictly applicable only to the latitude of Dunsink.

III. *Rotation of the field due to displacement of the polar axis.*—Let us now neglect for the time any effects of refraction and suppose that the stars are describing uniformly circles about the true pole. Let the instrumental pole P′ be at a distance e from the true pole P in hour-angle H.

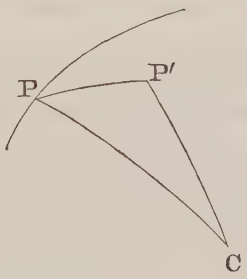

FIG. 1.

Let C be the projection of the centre of a plate in hour-angle h and instrumental polar distance p. Let the angle PCP′ be θ. The projection of CP′ on the plate will be what we may call the instrumental north and south line, while the projection of CP will be the true north and south line. And the angle θ varies with h. There will therefore be a rotation of the field during exposure measured by the value of $\frac{d\theta}{dh}$.

We have

$$\sin (h - \mathrm{H}) \sin e = \sin p \sin \theta,$$

Differentiating

$$\frac{d\theta}{dh} = \frac{\sin e \cos (h - \mathrm{H})}{\sin p \cos \theta}$$

and except for regions close to the pole we may put

$$\cos \theta = \text{unity, and } \frac{d\theta}{dh} = \frac{\sin e \cos (h - \mathrm{H})}{\sin p}$$

The values of this expression, in units of circular measure per hour, for the case $e = 100''$ are given in the following table :—

TABLE V.

Rotation per hour of the Field when the Instrumental Pole is displaced 100″ *from the True Pole.*

		$h-\mathrm{H}$				
Decl.	0°	0^h.	1^h.	2^h.	3^h.	4^h.
		·000127	·000123	·000110	·000090	·000063
	+10	129	124	112	91	64
	+20	135	130	117	96	68
	+30	147	142	127	104	73
	+40	166	160	144	117	83
	+50	198	191	171	140	99
	+60	·000254	·000245	·000220	·000180	·000127

The trail of a star whose coordinates on the plate are $(\xi\, \eta)$ will then be $-\eta \frac{d\theta}{dh}$ and $+\xi \frac{d\theta}{dh}$ in the two coordinates.

For example, if $\xi = \eta = \tan 1°$, and the declination is $+60°$, the corresponding values of $-\eta \frac{d\theta}{dh}$ or $+\xi \frac{d\theta}{dh}$ are

$$\mp 0''{\cdot}91 \quad \mp 0''{\cdot}88 \quad \mp 0''{\cdot}79 \quad \mp 0''{\cdot}65 \quad \mp 0''{\cdot}46$$

The effect, therefore, on stars near the edge of the field is by no means inconsiderable ; and displacements of the instrumental pole as large as some of those in Table III. are clearly out of the question except for short exposures or a very small field.

IV. *Rotation and distortion of the field, due to variation of the refraction with the time.*—Up to this point we have considered the effects of refraction upon a central guiding star and the consequent errors in the following. We have now to examine the effects upon the surrounding field.

The most appropriate refraction formulæ for use in this

examination are those derived by Professor Turner for rectangular coordinates (*Monthly Notices*, 1894, vol. liv. p. 19).

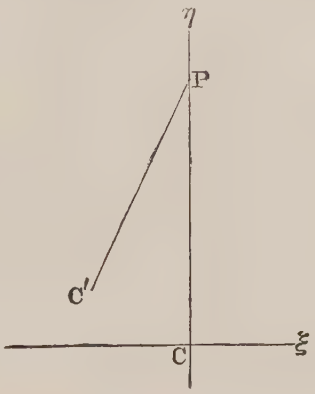

Fig. 2.

Let $C\xi$, $C\eta$ be the axes of coordinates of a plate, the projection P on the plate of the pole lying in $C\eta$. Let (X, Y) be the coordinates of the projection of the zenith ; β_0, the constant of refraction.

Then if $(\xi\eta)$ be the coordinates of the projection of a star's true place those of the apparent place as affected by refraction are $(\xi+\Delta\xi,\ \eta+\Delta\eta)$, where

$$\Delta\xi = \beta_0\,[X-(1+X^2)\,\xi - XY\eta]$$
$$\Delta\eta = \beta_0\,[Y-XY\xi-(1+Y^2)\,\eta]$$

neglecting small quantities of the second order.

If now the clock is rated to sidereal time, and the instrument is adjusted to the true pole, the path of the projection of C on the sky will be that of a star unaffected by refraction. Owing to the variation of the refraction with the hour-angle all the stars will trail upon the plate, and the expressions for the rate of trail in the two coordinates are

$$\frac{d}{dh}\,\Delta\xi = \beta_0\left[\frac{dX}{dh} - 2X\frac{dX}{dh}\xi - \left(X\frac{dY}{dh}+Y\frac{dX}{dh}\right)\eta\right]$$
$$\frac{d}{dh}\,\Delta\eta = \beta_0\left[\frac{dY}{dh} - \left(X\frac{dY}{dh}+Y\frac{dX}{dh}\right)\xi - 2Y\frac{dY}{dh}\eta\right]$$

These expressions may be conveniently divided into two :—

The first terms of each, $\beta_0\dfrac{dX}{dh}$ and $\beta_0\dfrac{dY}{dh}$, are the trail of a star whose true position unaffected by refraction would be at C.

This is our guiding star, and these terms indicate a translation of the field as a whole.

The remaining terms of the expressions

$$-\beta_0\left[2X\frac{dX}{dh}\xi+\left(X\frac{dY}{dh}+Y\frac{dX}{dh}\right)\eta\right]$$

$$-\beta_0\left[\left(X\frac{dY}{dh}+Y\frac{dX}{dh}\right)\xi+2Y\frac{dY}{dh}\eta\right]$$

involve ξ and η, and indicate a distortion of the field.

The guiding star is displaced by refraction from C to C′, and the position of C′ is constantly varying. The telescope must be made to follow it, and the north and south line on the plate is always C′P. The angle between this line and CP, which we may call the original axis of η, will therefore be continually changing, and a rotation of the field results.

We have then to evaluate two separate effects, a rotation and a distortion of the field.

A. *Rotation of the field.*—If the angle CPC′$=\phi$, and the N.P.D. of C is P, we have

$$\tan\phi=\frac{\beta_0 X}{\tan P}$$

very nearly, except for regions close to the pole. And ϕ is small,

$$\therefore\frac{d\phi}{dh}=\frac{\beta_0}{\tan P}\frac{dX}{dh}\text{ nearly.}$$

If c is the colatitude, h the hour-angle of the centre of the plate, the coordinates of the projection of the zenith on the plate are

$$X=\frac{\tan h\sin q}{\cos(P-q)}$$

$$Y=\tan(P-q)$$

where $\tan q=\tan c\cos h$.
Then

$$Y=\frac{\tan P-\tan c\cos h}{1+\tan P\tan c\cos h}$$

and

$$\frac{dY}{dh}=\frac{\tan c\sin h\sec^2 P}{(1+\tan P\tan q)^2}$$

Also

$$\tan h=\frac{X\operatorname{cosec} P}{1-Y\cot P}$$

$$\therefore X=\sin P\tan h-\cos P\tan h\,.\,Y$$

and

$$\frac{dX}{dh}=(\sin P-Y\cos P)\sec^2 h-\cos P\tan h\frac{dY}{dh}$$

From these expressions the values of $\frac{dX}{dh}, \frac{dY}{dh}$ were calculated for every hour of hour-angle from 0^h to 4^h, and for every 10° of declination from 0° to +50°.

Substituting then the values of $\frac{dX}{dh}$ in the equation $\frac{d\phi}{dh} = \frac{\beta_0}{\tan P}\frac{dX}{dh}$, the quantities of the following table were obtained:

TABLE VI.

Values of $\beta_0 \frac{dX}{dh} / \tan P$ *expressed in Circular Measure per hour.*

Decl.	Hour-angle.				
	0^h.	1^h.	2^h.	3^h.	4^h.
0°	·0000000	·0000000	·0000000	·0000000	·0000000
+10	108	114	133	176	280
+20	196	203	227	276	379
+30	284	292	313	356	443
+40	390	397	414	447	502
+50	·0000544	·0000544	·0000554	·0000571	·0000592

The trail of a star $(\xi\eta)$ due to this rotation will then be $+\eta\frac{d\phi}{dh}$ and $-\xi\frac{d\phi}{dh}$ in the two coordinates.

The rotation is in the opposite sense to that due to the elevation of the pole, and will therefore tend to counteract it. For declinations above 40°, with a suitable elevation of the pole, the compensation is fairly good, and the resultant rotation of the field is negligible. But near the equator the rotation due to the refraction is small, and that due to the elevation of the pole is able to produce its full effect.

B. *Distortion of the field.*—If we put

$$-\beta_0 2X\frac{dX}{dh} = A$$

$$-\beta_0\left(X\frac{dY}{dh}+Y\frac{dX}{dh}\right) = B$$

$$-\beta_0 2Y\frac{dY}{dh} = C$$

the trails of a star $(\xi\eta)$ tending to distort the field are at the rates $A\xi+B\eta$, $B\xi+C\eta$ in the two coordinates.

The values of A, B, and C are given in the following table to three significant figures, and are expressed in units of circular measure per hour :—

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TABLE VII.

Values of A, B, and C.

Decl.		Hour-angles.				
		0^h.	1^h.	2^h.	3^h.	4^h.
0°	A	−·0000000	−·0000426	−·000114	−·000297	−·00103
	B	·0000955	·000114	·000184	·000407	·00134
	C	·0000000	·0000709	·000190	·000494	·00171
+10	A	·0000000	·0000284	·0000700	·000154	·000384
	B	·0000560	·0000645	·0000976	·000186	·000458
	C	·0000000	·0000336	·0000858	·000199	·000532
+20	A	·0000000	·0000214	·0000497	·0000971	·000198
	B	·0000339	·0000391	·0000576	·000102	·000214
	C	·0000000	·0000178	·0000443	·0000970	·000228
+30	A	·0000000	·0000176	·0000388	·0000691	·000123
	B	·0000200	·0000236	·0000354	·0000614	·000169
	C	·0000000	·0000097	·0000241	·0000520	·000115
+40	A	·0000000	·0000156	·0000330	·0000549	·0000854
	B	·0000100	·0000127	·0000215	·0000393	·0000731
	C	·0000000	·0000047	·0000124	·0000280	·0000620
+50	A	·0000000	·0000147	·0000301	·0000469	·0000656
	B	·0000018	·0000041	·0000115	·0000255	·0000488
	C	−·0000000	−·0000011	−·0000043	−·0000129	−·0000329

From this table the rate of trail in each coordinate at any point of the plate may be easily calculated.

For in seconds of arc per hour

$$\frac{d}{dh}\Delta\xi = \frac{1}{\sin 1''}(A\xi + B\eta)$$

$$\frac{d}{dh}\Delta\eta = \frac{1}{\sin 1''}(B\xi + C\eta)$$

where (ξ, η) are the coordinates of any point on the plate expressed in circular measure.

Consider, as an example, the case of a plate 2° square, with centre 10° N. and 3^h from the meridian. If $\xi=\eta=\tan 1°$

$$\frac{A\xi}{\sin 1''} = -0''{\cdot}55 \qquad \frac{B\xi}{\sin 1''} = -0''{\cdot}67 \qquad \frac{C\eta}{\sin 1''} = -0''{\cdot}72$$

and

at the point		the rate of trail per hour is in ξ	in η
$\xi=\eta=\tan 1°$		$-1''{\cdot}22$	$-1''{\cdot}39$
$\xi=\tan 1°$	$\eta=0$	$-0''{\cdot}55$	$-0''{\cdot}67$
$\xi=\tan 1°$	$\eta=-\tan 1°$	$+0''{\cdot}12$	$+0''{\cdot}05$
$\xi=0$	$\eta=\tan 1°$	$-0''{\cdot}67$	$-0''{\cdot}72$

A star at the first point will then be trailing inwards at the rate of nearly 2″ per hour ; and during a long exposure the star discs in this and in the opposite corner of the plate will be very seriously distorted.

In this way was calculated the following table :—

TABLE VIII.

Values of $\frac{d}{dh}\Delta\xi$, $\frac{d}{dh}\Delta\eta$, *being Rates of Trail per hour in each Coordinate for a Star at the corner* $\xi = \eta = \tan 1°$ *of the plate, for various Declinations and Hour-angles.*

Decl.	Hour angle.				
	0^h.	1^h.	2^h.	3^h.	4^h.
0°	−0″·35	−0″·56	−1″·07	−2″·54	−8″·53
	0·35	0·67	1·34	3·25	10·98
+10	0·20	0·33	0·60	1·22	3·03
	0·20	0·35	0·66	1·39	3·56
+20	0·12	0·22	0·39	0·72	1·48
	0·12	0·20	0·37	0·72	1·59
+30	0·07	0·14	0·27	0·47	0·87
	0·07	0·11	0·22	0·41	0·84
+40	0·04	0·11	0·20	0·34	0·57
	0·04	0·07	0·12	0·24	0·48
+50	0·01	0·06	0·15	0·26	0·42
	0·01	0·01	0·06	0·14	0·30

These results appear to me to throw some doubt on Dr. Rambaut's belief that photographs suitable for accurate measurement can be obtained six hours or more from the meridian (*Monthly Notices*, 1896, vol. lvii. p. 52). I have not considered the case of hour-angles greater than four hours, because I have shown elsewhere (*Monthly Notices*, 1898, vol. lviii. p. 442) that, with reasonable luck in the matter of weather, it should never be necessary to take photographs for stellar parallax much more than four hours from the meridian. The importance of making every effort to work at the smallest possible hour-angle is evident from the table.

V. *General conclusions.*—Stellar photographs generally fall into one of two classes :—

A. Short exposures, less than 10^m.

B. Long exposures, an hour and upwards.

The astrographic catalogue plates and plates for stellar parallax belong to class A.

It is extremely desirable that as little hand correction as possible should be given to these photographs, destined for the most accurate measurement. It will therefore be advisable to elevate

the pole and adjust the clock-rates in order to ensure good following. For general work at varying declinations and hour-angles an elevation of about 60″ will probably be found best. For work in narrow zones, and near the meridian, other values of the elevation are found. Except near the equator the rotations of the field tend to counteract one another, and near the meridian the distortion is not very serious.

Astrographic chart plates, and plates intended for the parallax of clusters and nebulæ, fall into class B. For them it is a delicate matter to balance the respective merits or demerits of good following, absence of hand correction, rotation or distortion of the field. Near the equator and on the meridian it is best to adjust to the true pole, and sacrifice easy following to freedom from rotation, since distortion is fairly small. Far from the meridian distortion becomes so large that rotation is comparatively a minor ill. But here a small elevation of the pole has little effect on the following.

In high declinations the rotations compensate one another, and the distortions are small. It is here that the advantages of elevation of the pole are most fully realised, and there seems to be no reason why long exposure photographs should not be susceptible of very accurate measurement.

It is, however, useless to attempt to frame any general rules. Each case must be decided after a weighing of conflicting interests. But there seemed to be a want of numerical data on which to form a judgment. The tables contained in this paper were therefore calculated as a preliminary to the work which is to be shortly undertaken with the new photographic equatorial of the Cambridge Observatory; and they are now published in the hope that they may be found of more extended use.

Cambridge Observatory:
1898 *June* 4.

A Diagram showing the Conditions under which Observations for the Determination of Stellar Parallax are to be made. By Arthur R. Hinks, B.A.

(*Communicated by Sir R. S. Ball.*)

The diagram which is here described was constructed to facilitate the arrangement of the stellar parallax work to which the Director proposes to devote the new photographic equatorial of the Cambridge Observatory.

It shows for a star of any R.A. between the limits of declination 0° and +60°

a. The two days of the year on which the parallactic displacement of the star is a maximum.

b. The mean time on those days at which it is most favourably situated for observation.

c. The hour-angle and zenith distance at which it is then to be observed.

The diagram thus serves as a guide in the choice of seasons for work on any star ; and also as a guide to the order in which the photographs of all the stars in a night's working list should be taken, to avoid as far as possible large zenith distances.

I publish it now in the hope that the idea of such a diagram and the methods of its construction may be useful to others engaged in stellar parallax work.

The displacement of a star due to annual parallax is greatest at the two moments when the star is 90° from the Sun.

If then H and D are the hour-angle and declination of the Sun, h and δ the hour-angle and declination of a star, a condition for maximum parallactic displacement is

$$\text{Sin D sin } \delta + \cos \text{D} \cos \delta \cos (\text{H} - h) = 0 \quad . \quad . \quad . \quad . \quad . \quad (1)$$

I have assumed that it is possible to begin photographing when the Sun is 10° below the horizon. The corresponding hour-angle of the Sun, H, is given by the equation

$$\text{Cos } 100^\circ = \cos c \sin \text{D} + \sin c \cos \text{D} \cos \text{H} \quad . \quad . \quad . \quad . \quad (2)$$

where c is the colatitude of the observatory.

From equation (2) we find H the apparent time, and thence the mean time, at which it is possible to begin photographing on any day in the year. If in (1) we substitute this value of H and the corresponding value of D, we have a relation connecting the declinations and hour-angles at the time of beginning work of all the stars which are then at maximum parallactic displacement.

The construction of the diagram, therefore, proceeded as follows :—

The hour-angles H of the Sun when it reaches the zenith distance 100° were calculated from equation (2) for every fifteenth day of the year.

These values of H and the corresponding values of D were then substituted in equation (1), and the values of h obtained for every 10° of declination δ, from the equator to +60°. These values of h were plotted on millimetre paper, with dates as abscissæ and hour-angles as ordinates ; and the curves drawn through the corresponding points of each set form the declination curves of the diagram.

Again, from the value of H we find the sidereal time at which the Sun reaches the zenith distance 100° on any day ; and then we can lay down a series of points representing the hour-angles at this instant for stars of each hour of R.A. This was done for each fifteenth day ; and the curves drawn through corresponding points of each series are the R.A. curves of the diagram.

Along the bottom were laid down for various dates the mean

times corresponding to the calculated apparent times H at which the Sun reaches the zenith distance 100°.

In a precisely similar manner the curves were laid down which show the conditions under which the star is to be observed in the morning, when it is at the opposite apsé of its parallactic ellipse.

Finally, at the right-hand side of the diagram, the curves were drawn which give for every 10° of declination the zenith distances corresponding to the hour-angles.

The use of the diagram is best illustrated by examples.

Suppose we require to observe for parallax a star in R.A. 10^h; Decl. +40°. The intersection of the corresponding curves gives May 7 as the date for the evening observation. The scale of mean times at the bottom shows that the Sun reaches the zenith distance 100° at about $8^h\ 45^m$ M.T. on that day. This is the time at which we can begin photographing. The scale of ordinates shows that the hour-angle of the star is then 29° W.; and the curves on the right give 23° as the corresponding zenith distance.

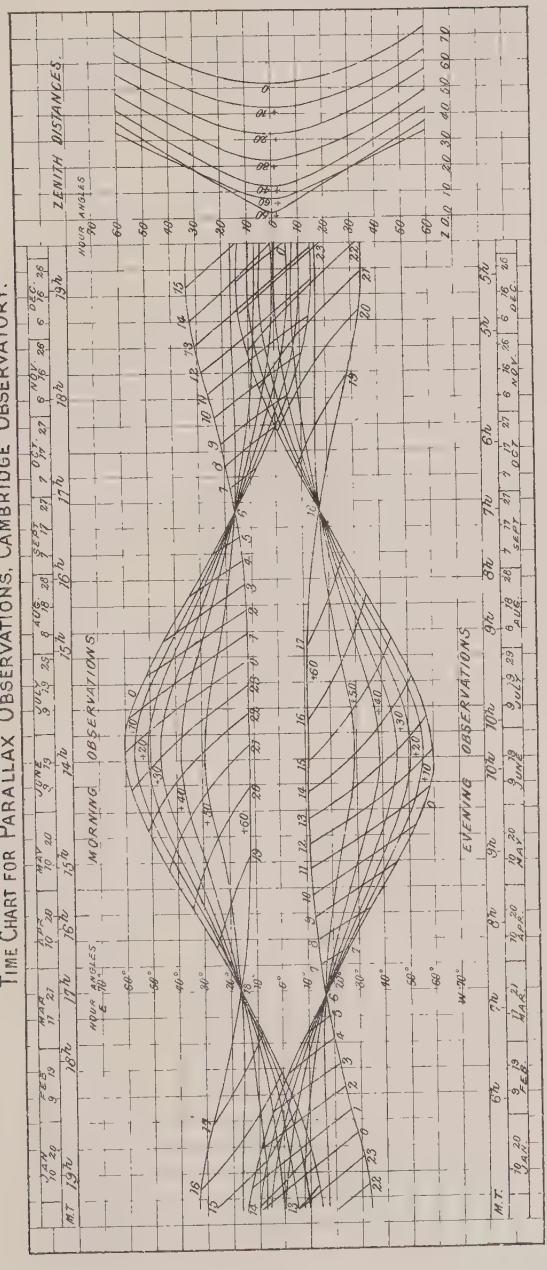
On the same evening we can see that stars whose positions are R.A. 9^h Decl. 0°, and R.A. 11^h Decl. +60° might be in the working list. At $8^h\ 45^m$ their respective zenith distances are 63° and 10°. We should therefore start at the earliest possible moment on the equatorial star. When that was done—in half an hour, say—we could get the star 40° N. at about 27° Z.D.; and after another half-hour the star 60° N. at 13° Z.D.

A simple inspection of the diagram gives then the conditions of hour-angle and zenith distance at the earliest times in the evening and the latest times in the morning at which the stars in the night's working list can be photographed. And in general these limiting times will be the best times for the work, since the zenith distances are then the smallest possible consistent with the conditions under which the work is to be done.

In some cases, however, the diagram shows that this is not true. For example, stars within 30° of the equator which are to be observed in the mornings in midwinter have passed the meridian when the Sun attains the zenith distance 100°; and it will be better to take them a little earlier, when they are on the meridian.

Granted fine weather, and the possibility of starting work very early in the evening and continuing it very late in the morning, it will be practicable, as the diagram shows, to avoid having to work at any hour-angle much exceeding three hours, except in the middle of summer, when the hour-angles for equatorial stars become almost prohibitively large. An extreme case is that of a star on the equator in R.A. 12^h, which in the case of the evening observation could not be photographed before it had reached the zenith distance 73°.

The diagram as published reproduces only too faithfully one or two irregularities, due to unskilful drawing of the original. All the declination curves should of course intersect in two


TIME CHART FOR PARALLAX OBSERVATIONS, CAMBRIDGE OBSERVATORY.
ZENITH DISTANCES.
HOUR ANGLES
MORNING OBSERVATIONS
EVENING OBSERVATIONS
M.T.
W.-70°
+70°

points corresponding to the two equinoxes, which correspondence is in the drawing not quite exact. The accuracy of the drawing is, however, amply good enough for the purpose for which it was made.

Cambridge Observatory:
1898 *May* 28.

Revised Catalogue of the Stars of the IV. Type.

By the Rev. T. E. Espin, M.A.

The arrangement of the present catalogue is identical with that of the first catalogue of the Stars of the IV. Type, published in the *Monthly Notices*, vol. xlix. p. 364. The letter H has been used to signify the stars detected at the Harvard Observatory either visually, or by the photo-telescopes at Harvard and Arequipa. Professor Pickering has kindly sent me a complete list of these, containing some that have not hitherto been published. Also some notes on stars in the first catalogue, which, together with my own observations, have caused the removal of several stars from the list, and their being assigned to Type III. One star, No. 191, in the present catalogue, of which Professor Pickering writes, "Omit, not confirmed by the photographs," I have still ventured to retain, as although the star is not above 11·5 mag. I have no doubt as to the correctness of the earlier Harvard observations. I have observed it on several occasions, and although faint the type seemed certain. The stars in Column II. marked Es. and followed by a number, are stars not in the B.D. The other abbreviations in Column VI. are as follows: Se., Secchi; Du., Dunér; D'A., D'Arrest; P., Pechüle; V., Vogel.

The following is a general summary of the catalogue, the first column giving the limiting magnitude, the second the number of stars north of the equator, the third the number of stars south, and the last the totals, the variable stars being entered according to their maximum magnitude:—

Mag.	N.	S.	Total.
to 6·0	3	4	7
6·1 „ 7·0	12	11	23
7·1 „ 8·0	19	20	39
8·1 „ 9·0	51	25	76
Below 9·0	69	11	80
Mag. not given	1	11	12
Total	155	82	237

Excluding variables our knowledge of the number of stars of Type IV. is probably complete for the N. heavens as far as 8·9, and for the southern heavens as far as 8·5. The catalogue

contains twenty-eight variables to which letters have been assigned : twenty-two are north and six south. It would appear that almost all the stars of Type IV. are subject to fluctuations in brightness, though the red colour makes it not easy to decide when the variation is small.

No.	B.D. &c.	R.A. (1900). h m	Decl. (1900). ° ′	Mag.	Authority.
1	+49°,41	0 12·3	+49 44	9·0	H.
2	+43°,53	14 6	+44 9	8·2	Se.
3	Es. 832	16·8	+58 36	9 9	Es.
4	+53°,66	19 1	+53 44	9·3	H.
5	+34°,56	22·2	+35 2	8·1	Du.
6	+63°,56	25·5	+63 19	9·3	Es.
7	W Cassiopeiæ	49·0	+58 1	Var.	„
8	+62°,211	1 2·6	+62 27	9·5	„
9	+25°,205	10·6	+25 15	7·0	D'A.
10	R Sculptoris	22·4	−33 3	Var.	H.
11	+59°,274	26·8	+60 8	9·0	Es.
12	Es. 230	27·1	+57 14	9·2	„
13	Es. 1076	37·7	+60 7	9·8	„
14	Es. 1181	37·8	+61 7	9 6	„
15	+53°,379	38·7	+53 28	9·4	H.
16	X Cassiopeiæ	49·8	+58 46	Var.	Es.
17	Es. 1084	2 3·4	+63 9	9·7	„
18	+11°,305	9·6	+11 47	8 9	H.
19	+51°,575	19·8	+51 37	9·0	Es.
20	−10°,513	30·2	− 9 53	8·0	H.
21	+38°,525	32·1	+38 44	9·4	Es.
22	+58°,501	32·3	+59 10	9·5	„
23	V Persei	43·3	+56 34	Var.	H.
24	+57°,647	43·6	+57 26	8·9	Du.
25	+57°,702	3 3·7	+57 31	7·9	H.
26	+47°,783	6·7	+47 27	9·0	Es.
27	+43°,726	20·9	+43 50	8·9	„
28	U Cameli	33·2	+62 19	Var.	Du.
29	+51°,762	34·1	+51 11	8·9	Es.
30	C.Z.C. $3^{h}\cdot1404$	46·7	−43 50	8·5	H.
31	Es. 1111	51·3	+60 33	9·2	Es.
32	+61°,667	57·2	+61 31	7·5	„
33	+50°,920	4 3·9	+51 5	9·5	„
34	+50°,961	9·0	+50 22	9·5	„

No.	B.D. &c.	R.A. (1900). h m	Decl. (1900). ° ′	Mag.	Authority.
35	+48°,1083	4 13·6	+48 56	9·5	Es.
36	T Cameli	30·4	+65 57	Var.	,,
37	Es. 985	32·6	+41 23	9·2	,,
38	+42°,1046	39·6	+42 29	9·5	,,
39	+67°,350	40·8	+68 0	7·0	Se.
40	+34°,911	42·6	+34 49	8·8	Es.
41	C.G.C. 5429	43·8	−36 23	7·6	H.
42	+28°,707	45·2	+28 21	8·1	Se.
43	+38°,955	45·8	+38 20	8·8	Es.
44	+22°,770	47·8	+22 37	9·2	,,
45	R Leporis	55·0	−14 57	Var.	D'A.
46	+50°,1112	55·6	+50 29	8·9	Es.
47	+0°,939	5 0·2	+ 1 2	6·0	Se.
48	+38°,1038	2·5	+38 54	9·5	Es.
49	+45°,1053	2·8	+46 2	9·5	,,
50	−5°,174	4·9	− 5 39	8·7	Du.
51	+35°,1046	12·5	+35 41	8·9	Es.
52	+32°,957	15·3	+32 25	9·3	,,
53	S Aurigæ	20·5	+34 4	Var.	Du.
54	+7°,929	27·8	+ 7 4	8·2	Es.
55	S Cameli	30·2	+68 45	Var.	,,
56	C.G.C. 6519	31·7	−25 48	7·5	H.
57	+24°,898	32·4	+24 57	9·5	Es.
58	+17°,979	35·5	+17 29	8·0	H.
59	+24°,943	39·1	+24 23	8·5	Du.
60	+20°,1083	39·7	+20 39	7·7	,,
61		40·4	−46 30	7½	P.
62	+44°,1288	41·2	+44 48	9·2	Es.
63	+30°,1014	41·7	+30 35	8·5	,,
64	+26°,1117	6 4·7	+26 2	7·4	D'A.
65	+27°,1024	7·3	+27 12	9·0	Se.
66	+29°,1177	13·2	+29 31	9·5	Es.
67	V Aurigæ	16·3	+47 43	Var.	,,
68	+3°,1214	17·1	+ 3 28	9·0	,,
69	+25°,1250	17·8	+25 4	9·5	,,
70	+14°,1283	19·8	+14 47	6·5	Se.
71	Es. 243	20·3	+19 8	Var.?	Es.
72	+38°,1539	29·7	+38 32	6·3	Se.
73	Es. 1142	33·3	+22 42	9·4	Es.

No.	B.D. &c.	R.A. (1900). h m	Decl (1900). ° ′	Mag.	Authority.
74	+31°,1388	6 35·7	+31 33	8·1	Es.
75	+3°,1381	39·4	+ 3 25	9·3	H.
76	Es. 63	42·4	+ 0 47	9·6	Es.
77	−6°,1786	47·5	− 7 0	9·1	,,
78	−4°,1708	48·2	− 4 27	9·0	,,
79	C.G.C. 8670	51·3	−42 14	6·7	H.
80	+6°,1462	53·0	+ 6 18	8·0	Es.
81	Es. 1144	55·3	+68 19	10	,,
82	−3°,1685	56·0	− 3 7	7·7	,,
83	−7°,1742	7 2·1	− 7 24	8 3	,,
84	−11°,1805	3·4	−11 46	7·6	Se.
85	+14°,1594	6·6	+14 53	9 0	Es.
86	+25°,1643	14·9	+25 10	9·2	,,
87	−3°,1873	18·1	− 4 2	9·2	H.
88	−3°,1885	19·9	− 4 2	8·7	Es.
89	−2°,2101	20·2	− 2 57	9 0	,,
90	+24°,1685	25·8	+24 43	8·2	,,
91	+2°,1715	31·3	+ 2 18	9·3	,,
92	U Canis Min.	35·9	+ 8 37	Var.	V.
93	+5°,1797	43·4	+ 5 40	9 0	Es.
94	C.G.C. 10488	53·5	−49 43	8	P.
95	−12°,2289	57·5	−12 44	10	H.
96	−38°,4049	8 1·7	−38 29	—	,,
97	−22°,2160	3·1	−22 38	8 6	,,
98	+3°,1958	14·9	+ 3 5	8 3	Es.
99	−17°,2442	15·2	−17 57	9·1	H.
100	C.G.C. 11890	42·4	−29 21	7 9	,,
101	+17°,1973	49·7	+17 37	6·5	D'A.
102	T Cancri	51·0	+20 14	Var.	Es.
103	+14°,2048	9 8·3	+14 37	8 8	H.
104		13·5	−65 49	—	,,
105	−1°,2312	45·9	− 1 33	8·9	,,
106	−22°,2739	46·4	−22 33	6·6	Se.
107		51·3	−41 7	7·5	P.
108	C.G.C. 13699	57·9	−59 49	7¾	,,
109	C.G.C. 13396	10 7·5	−34 50	7¼	,,
110	C.G.C. 14440	30 8	−39 3	5 9	H.
111	U Hydræ	32·6	−12 52	Var.	Se.
112	+68°,617	38·1	+67 56	6·2	Du.

No.	B.D. &c.	R.A. (1900). h m	Decl. (1900). ° ′	Mag.	Authority.
113		10 42·5	−65 5	—	H.
114	V Hydræ	46·8	−20 43	Var.	Se.
115	+69°,644	56·5	+69 47	8·9	H.
116	C.Z.C. 11h·129	11 2·9	−54 35	9	„
117	C.Z.C. 11h·742	11·2	−57 23	9	„
118	−13°,3407	30·7	−14 2	8·5	„
119	C.G.C. 15946	35 0	−72 0	8·5	„
120		55·3	−54 33	—	„
121		12 9·4	−50 58	—	„
122		19·3	−48 51	—	„
123	+1°,2694	20·1	+ 1 19	8·1	Se.
124	−37°,7905	24·0	−37 42	8·8	H.
125	+46°,1817	40·4	+45 59	5·5	Se.
126	+66°,780	52·5	+66 32	7·3	„
127	+38°,2389	54·7	+38 21	8·6	H.
128	C.Z.C. 13h·717	13 13·4	−73 55	8½	„
129	C.G.C. 18157	15·5	−63 42	8	„
130	C.Z.C. 13h·1490	26·4	−53 19	9·6	„
131	C.G.C. 18947	51·6	−55 51	8	„
132	C.G.C. 19254	14 7·4	−53 28	7·5	„
133	C.G.C. 19416	15·7	−49 24	8½	„
134	C.G.C. 19745	29·5	−42 56	8·5	„
135		52·2	−53 0	—	„
136	C.G.C. 20554	15 4·8	−69 42	6·2	„
137	C.G.C. 20937	21·9	−24 49	7·6	„
138	V Coronæ	45·9	+39 52	Var.	Du.
139	RR Herculis	16 1·5	+50 46	Var.	Es.
140		21·1	−43 26	—	H.
141	V Ophiuchi	21·2	−12 12	Var.	Du.
142		39·8	−67 36	—	H.
143	C.G.C. 23005	54·3	−54 55	8½	„
144		17 11·6	−45 52	—	„
145	−19°,4644	23·9	−19 23	7·8	Du.
146	C.G.C. 23935	34·7	−57 40	7·0	H.
147	−18°,4634	39·1	−18 37	8·5	Du.
148	C.Z.C. 17h 2657	40·8	−35 40	8·0	H.
149	T Draconis	54·9	+58 14	Var.	Es.
150	−39°,12196	58·2	−39 20	9·0	H.
151	+9°,3576	18 4·0	+ 9 26	9·4	„

No.	B.D. &c.	R.A. (1900). h m	Decl. (1900). °	Mag.	Authority.
152	−19°,4907	18 7·8	−19 16	9·5	Es.
153	Holden 150	7·9	−19 7	—	„
154	−13°,4918	12·7	−13 29	9·2	„
155	−15°,4923	13·6	−15 39	9·0	H.
156	−38°,12843	23·4	−38 29	9·5	„
157	+4°,3779	26·4	+ 4 19	9·3	Es.
158	T Lyræ	28·9	+36 55	Var.	Se.
159	−7°,4633	31·6	− 7 41	9·0	Es.
160	+32°,3160	33·6	+33 1	9·3	„
161	C.Z.C. 18h·1935	35·2	−51 51	9	H.
162	+36°,3243	39·4	+36 52	7·5	Se.
163	−8°,4726	44·9	− 8 1	7·1	Du.
164	+0°,4046	52·4	+ 0 19	9·2	Es.
165	+14°,3729	54·0	+14 14	9·0	Du.
166	+10°,3764	57·5	+10 6	9·5	Es.
167	V Aquilæ	59·1	− 5 50	Var.	V.
168	C.G.C. 26129	59·7	−38 17	8·3	H.
169	−16°,5272	19 13·4	−16 6	6·8	„
170	+10°,3857	13·8	+10 20	9·5	Es.
171	U Lyræ	16·6	+37 41	Var.	„
172	+76°,734	25·1	+76 22	6·5	Se.
173	+45°,2906	25·8	+45 50	8·6	Es.
174	−16°,5360	28·6	−16 35	7·2	Se.
175	+32°,3522	37·1	+32 23	8·0	Du.
176	−18°,5480	40·6	−18 24	9·1	H.
177	Es. 1021	42·9	+15 48	9·5	Es.
178	+85°,332	43·8	+85 9	9·2	H.
179	+24°,3902	46·3	+24 41	9·2	Es.
180	Es. 415	46·7	+14 45	9·8	„
181	+43°,3425	54·0	+43 59	8·2	Du.
182	−7°,5141	55·7	− 7 39	9·8	H.
183	+9°,4369	56·3	+ 9 14	8·7	„
184	Es. 181	57·1	+30 33	9·5	Es.
185	+20°,4390	58·9	+20 48	9·4	„
186	+40°,4001	20 0·9	+40 9	9·5	„
187		5·1	+35 31	—	H.
188	+41°,3632	6·3	+41 12	Var.	Es.
189	+47°,3031	6·4	+47 33	Var.	Du.
190	RY Cygni	6·6	+35 38	Var.	H.

No.	B.D. &c.	R.A. (1900). h m	Decl. (1900). ° ′	Mag.	Authority.
191	Pickering No. 38	20 7·3	+35 48	(11)	H.
192	RS Cygni	9·8	+38 28	Var.	Du.
193	−21°,5672	11·2	−21 37	Var.	Se.
194	Es. 1170	11·8	+36 47	11	Es.
195	Es. 900	12·4	+37 34	10	,,
196	Es. 417	13·2	+49 38	Var.	,,
197	Es. 902	13·3	+36 36	11·5	,,
198	+37°,3876	14·8	+37 5	9·5	,,
199	U Cygni	16·5	+47 34	Var.	Se.
200	+35°,4077	17·4	+35 18	9·5	Es.
201	+37°,3903	17·9	+37 13	9·4	,,
202	RW Cygni	25·2	+39 39	Var.	,,
203	−12°,5755	26·2	−12 13	9·2	H.
204	+40°,4210	27·4	+40 11	9·4	Es.
205	+68°,1140	36·1	+68 12	8·8	,,
206	V Cygni	38·1	+47 47	Var.	V.
207	+45°,3271	43·5	+45 41	Var.	Es.
208	+32°,3954	45·2	+32 51	9·4	,,
209	Es. 1172	21 6·5	+32 57	9·1	,,
210	C.G.C. 29232	13·6	−45 27	6·0	H.
211	Es. 923	14·4	+51 49	9·5	Es.
212	C.G.C. 29252	15·2	−70 10	6·8	H.
213	+41°,4114	18·6	+41 58	9·5	Du.
214	+61°,2134	23·3	+62 8	8·8	Es.
215	+49°,3535	25·8	+49 53	9·4	,,
216	+47°,3429	26·1	+48 7	9·5	,,
217	S Cephei	36·5	+78 10	Var.	Du.
218		36·6	−65 30	—	H.
219	+34°,4500	37·8	+35 3	6·2	D'A.
220	RV Cygni	39·1	+37 34	Var.	Se.
221	+53°,2693	40 7	+53 15	9·2	Es.
222	Es. 931	43·0	+52 5	9·5	,,
223	+52°,3036	43·6	+52 13	9·3	,,
224	+49°,3673	51·5	+50 1	9·1	Du.
225	C.G.C. 30526	22 16·6	−46 27	6·7	H.
226	Es. 1046	24 8	+64 23	10	Es.
227	+60°,2432	40·4	+61 12	8·9	,,
228	+54°,2863	43·6	+54 38	9·5	,,
229	+45°,4121	57·2	+45 21	9·5	,,

No.	B.D. &c.	R.A. (1900). h m	Decl. (1900). ° ′	Mag.	Authority.
230	Es. 1048	23 6·0	+60 43	9·0	Es.
231	−21°,6376	6·3	−21 32	9	H.
232	+48°,4051	22·2	+48 58	9·3	Es.
233	+2°,4709	41·3	+2 56	6·2	Se.
234	+5°,5223	44·0	+5 50	8·7	H.
235	+60°,2634	48·0	+60 27	9·0	Es.
236	+59°,2810	56·2	+59 48	7·8	Du.
237	+42°,4827	59 5	+43 0	8·4	Es.

Further Researches on the Orbit of γ Lupi = h 4786.

By T. J. J. See, A.M., Ph.D. (Berlin).

In the *Monthly Notices* for 1897 November I have indicated the general nature of the orbit of *γ Lupi*. At the time that paper was prepared, it did not seem probable that a material improvement of the result announced could be expected for some twenty years; but a few observations made by Captain Jacob at a particularly opportune epoch, near the middle of the century and unknown to me at the time of my first investigation, have made possible a revision which defines the motion of this remarkable star with singular precision. We are thus enabled to determine a set of elements which are very satisfactory. Considering the small number of observations available, it must be conceded that this result is one of the happiest yet afforded by the history of double star astronomy. The excellence of the orbit derived from only a few observations is due to their advantageous distribution, which gives each measure a particularly decisive import. The singular and almost unique character of the orbit also renders its determination comparatively easy. We find that the system of *γ Lupi* is situated like that of 42 *Comæ Berenices*, and that all the motion takes place in a right line at position angles 93°·5—273°·5. An investigation of the areas described shows that the major axis of this system does not depart sensibly from the line of sight; and hence the determination of the elements is somewhat easier than in the case of 42 *Comæ Berenices*, where λ differs sensibily from 90°.

As nearly all the observations of *γ Lupi* were given in my former paper, I content myself here with adding those since unexpectedly discovered, and a few which Mr. Boothroyd and I have recently taken at Flagstaff.

t	θ_o	ρ_o	Remarks.
1853·125	274°·6	1″·14	1*n* Jacob. Discs in contact.
1853·130	272·4	0·98	1*n* Jacob. Preceding star certainly the least, but the difference is less than half a magnitude.
1856·171	275·4	0·75	3–2*n* Jacob. Magnitudes 4–4.
1877·411	Not divided		1*n* Melbourne Obs., 8-inch refractor.
·419	„ „		1*n* „ „
·422	Plainly elongated		1*n* „ „
1878·679	Not divided		1*n* „ „
1898·302	93·9	0·40	See. Clearly separated with black line between components.
·302	92·7	0·46	
·302	92·0	0·35	See. Excellent measures.
·302	93·9	0·35	
1898·302	93·4	0·46	Boothroyd.

The Melbourne observations were kindly communicated by Mr. Innes, of the Royal Observatory, Cape of Good Hope.

It is singular that Captain Jacob should have been able to measure this close object with a 5-inch telescope, and the result speaks well for his skill as an observer. It is needless to say that his observations now possess an interest scarcely inferior to those of Herschel himself.

As the south declination of this object is about 41°, our recent observations were made at an altitude of only about 12° or 13° ; yet under a singularly steady atmosphere the stars were well shown, and most of the time distinctly divided. The measures are therefore entitled to the highest confidence.

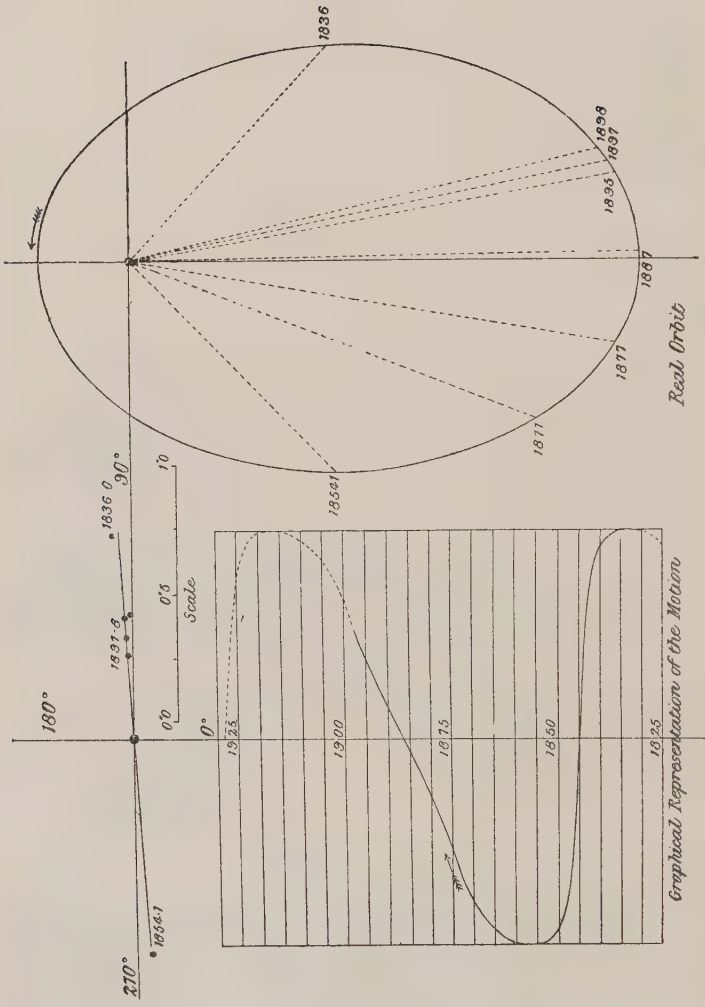
The following are the elements of γ *Lupi* = *h* 4786 :

P = 83·0 years	Ω = 93°·5
T = 1845·0	i = 90°·±
e = 0·70	λ = 90°·±
a = 1″·10	n = ±4°·3374

Apparent orbit, a right line in 93°·5—273°·5, 1″·56 in length.

In the accompanying table these elements are compared with the best means I could form from all the known observations of this star. As the elements are approximate, and the motion rather slow, I have taken the epochs merely to the nearest tenth year.

The figure shows the real and apparent orbits, and gives a graphical representation of the motion. The components are now steadily widening out, and ought soon to be visible to other southern observers.



Orbit of γ *Lupi* = h 4786.

The period of γ *Lupi* remains uncertain to the extent of possibly five years, but a longer correction to the above value seems altogether improbable.

Comparison of computed with observed places.

t	θ_o	θ_c	ρ_o	ρ_c	$\theta_o-\theta_c$	$\rho_o-\rho_c$	n	Observers.	Remarks.
1836·0	95°·3	93°·5	0″·78	0″·78	+1°·8	0″·00	16–9	Herschel	
1854·1	274·1	273·5	0·83	0·78	+0·6	0·05	5–3	Jacob	
1871·5		273·5		0·58			1	Russell	Not divided.
1877·5	270° ±	273·5		0·31	−3·5		1	Russell	Smaller end goes first.
1887·5		93·5		0·03			1	Pollock	Single in 11½ in.
1895·6		93·5		0·32			3	Sellors	Single
1897·1	92·4	93·5	0·34	0·36	−1·1	−0·02	2	See	
1897·1	90·4	93·5	0·47	0·36	−3·1	+0·11	1	Cogshall	
1898·3	93·1	93·5	0·39	0·40	−0·4	−0·01	2	See	
1898·3	93·4	93·5	0·46	0·40	−0·1	+0·06	1	Boothroyd	

The eccentricity of the orbit is quite high, and is comparatively well determined by the great inequality of the motion at the two apses. The value here found will probably not require a correction larger than ±0·05.

In closing I may perhaps observe that this remarkable system and 42 *Comæ* appear to differ from *Algol* variables chiefly in the equal luminosity of both components and the greater periods required for the revolution of their attendants.

Lowell Observatory, Flagstaff, Arizona:
1898 *May* 20.

Note on the Level Errors of the Cape Transit Circle.

By W. H. Finlay, M.A.

In vol. i. Part V. of the *Annals of the Cape Observatory* I discussed the variations of the adjustments of the transit circle from the time of its erection, 1856, up to 1882. The most interesting of these variations were those in level, which were shown to have a well-marked annual period, and which also, there was some reason to believe, were subject to a period of about 18 or 19 years.

As the results of 15 additional years are now available it may be of interest to put on record the behaviour of the level during that time.

In 1894 the error of azimuth had become inconveniently large. On May 16–17 both Ys were moved from their founda-

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tion plates, and the instrument was adjusted in azimuth. Corrections have been applied to the observations taken after this date to allow for the sudden change of level caused by this operation, and similarly in all cases where there was a change arising from mechanical interference with the instrument.

The following table gives the level errors from 1885 to 1897, formed in the same way and reduced to the same system as those given in the *Annals* for 1856–1884.*

Values of Level Error.

	1885.	1886.	1887.	1888.	1889.	1890.	1891.
Jan. 1	−3″·88	−3″·24	−3″·01	−3′·34	−2″·52	−2″·39	−1″·96
15	−3·90	−3·13	−2·93	−3·36	−2·58	−2·43	−2·09
Feb. 1	−3·70	−3·05	−2·98	−3·29	−2·66	−2·49	−2·07
15		−3·01	−3·08	−3·26	−2·71	−2·43	−2·14
Mar. 1		−3·00	−3·14	−3·29	−2·74	−2·43	−2·16
15		−3·07	−3·18	−3·16	−2·86	−2·48	−2·06
Apr. 1		−3·10	−3·44	−3·29	−3·02	−2·44	−2·10
15		−3·14	−3·54	−3·37	−3·10	−2·52	−2·10
May 1		−3·21	−3·73	−3·45	−3·02	−2·63	−2·18
15		−3·42	−3·63	−3·45	−3·11	−2·70	−2·26
June 1		−3·47	−3·77	−3·54	−3·10	−2·74	−2·35
15		−3·45	−3·72	−3·54	−3·17	−2·72	−2·39
July 1		−3·46	−3·80	−3·66	−3·19	−2·72	−2·53
15		−3·51	−3·76	−3·63	−3·17	−2·79	−2·44
Aug. 1		−3·51	−3·78	−3·32	−3·19	−2·80	−2·44
15	−4·08	−3·47	−3·78	−3·22	−3·12	−2·79	−2·47
Sept. 1	−3·94	−3·44	−3·64	−3·18	−2·94	−2·57	−2·36
15	−3·89	−3·40	−3·48	−2·97	−2·84	−2·38	−2·35
Oct. 1	−3·76	−3·29	−3·39	−2·80	−2·76	−2·36	−2·31
15	−3·79	−3·30	−3·38	−2·73	−2·74	−2·31	−2·12
Nov. 1	−3·63	−3·26	−3·32	−2·65	−2·51	−2·10	−2·10
15	−3·52	−3·15	−3·20	−2·50	−2·53	−2·12	−1·91
Dec. 1	−3·42	−3·12	−3·14	−2·52	−2·46	−2·03	−1·86
15	−3·36	−3·03	−3·27	−2·48	−2·43	−1·97	−1·84
Means	[−3·70]	−3·26	−3·42	−3·16	−2·85	−2·47	−2·19

Object-glass being re-polished

* It must be understood that the level errors given here are not the ones used in the ordinary reduction of observations. By occasional adjustment the level error in use has been kept small, but for my purpose corrections have been applied to allow for these adjustments, so as to exhibit the errors as they would have been without alteration from the beginning.

		1892.	1893.	1894.	1895.	1896.	1897.	1898.	Means.
Jan.	1	−1″80	−1″54	−1″49	−1″37	−1″61	−1″73	−2″22	−2″16
	15	−1·76	−1·54	−1·47	−1·40	−1·53	−1·75	−2·18	−2·16
Feb.	1	−1·76	−1·50	−1·40	−1·41	−1·49	−1·77	−2·33	−2·15
	15	−1·75	−1·41	−1·48	−1·35	−1·43	−1·75	−2·28	−2·15
Mar.	1	−1·75	−1·55	−1·47	−1·35	−1·30	−1·80	−2·29	−2·16
	15	−1·76	−1·48	−1·55	−1·31	−1·50	−1·95	−2·35	−2·19
Apr.	1	−1·79	−1·45	−1·69	−1·49	−1·62	−2·04	−2·52	−2·29
	15	−1·88	−1·60	−1·56	−1·61	−1·58	−2·16	−2·71	−2·35
May	1	−1·98	−1·71	−1·56	−1·64	−1·77	−2·19	−2·67	−2·43
	15	−2·10	−1·65	−1·58	−1·76	−1·77	−2·30	−2·77	−2·48
June	1	−2·13	−1·80	−1·99	−1·88	−1·80	−2·39	...	−2·58
	15	−2·21	−1·74	−1·98	−2·01	−1·79	−2·45	...	−2·60
July	1	−2·19	−1·73	−1·99	−1·90	−1·89	−2·46	...	−2·63
	15	−2·28	−1·78	−2·03	−1·99	−1·89	−2·47	...	−2·65
Aug.	1	−2·19	−1·77	−2·12	−1·95	−1·96	−2·51	...	−2·63
	15	−2·09	−1·85	−2·09	−1·91	−1·97	−2·45	...	−2·60
Sept.	1	−1·90	−1·78	−1·91	−1·99	−1·89	−2·50	...	−2·51
	15	−1·90	−1·74	−1·91	−2·01	−1·94	−2·59	...	−2·46
Oct.	1	−1·83	−1·75	−1·81	−1·81	−1·91	−2·57	...	−2·39
	15	−1·99	−1·64	−1·76	−1·79	−1·87	−2·47	...	−2·34
Nov.	1	−1·94	−1·68	−1·69	−1·84	−1·83	−2·45	...	−2·29
	15	−1·92	−1·51	−1·67	−1·71	−1·82	−2·44	...	−2·21
Dec.	1	−1·78	−1·41	−1·49	−1·72	−1·86	−2·39	...	−2·14
	15	−1·77	−1·44	−1·45	−1·61	−1·78	−2·29	...	−2·11
Means		−1·93	−1·61	−1·71	−1·74	−1·73	−2·24		

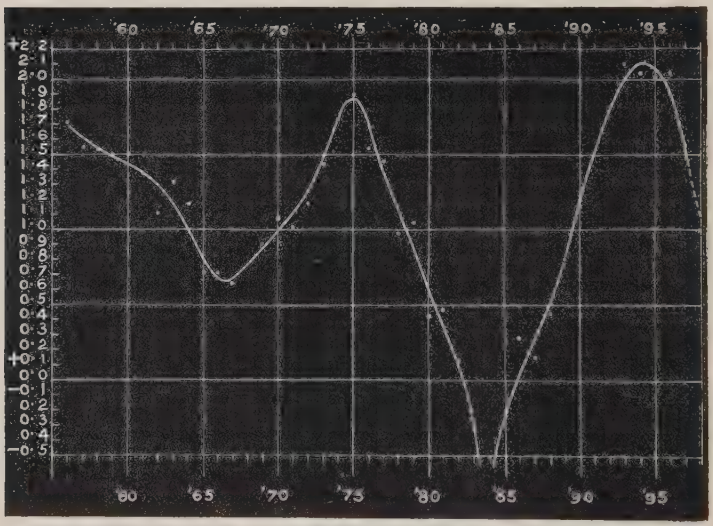
It is at once obvious from an inspection of the yearly means that for some years after erection there was a rapid sinking of the western pier, which has gradually diminished till at the present time it is almost insensible ; so that the assumption (made in my previous paper) of a uniform fall will not fit the later observations.

We might allow for this sinking in various ways, but I shall content myself with assuming that it proceeded in a geometrical progression of which the first term is 0·28 and ratio 0·93.

Correcting the yearly means, then, from the expression $4\ \{1-(0{\cdot}93)^n\}$, where n denotes the number of years from 1856, we obtain the following figures for the variation of level :—

Year.	Level Error.	Year.	Level Error.	Year.	Level Error.	Year.	Level Error.
	″		″		″		″
1856	+1·71	1867	+0·66	1878	+0·98	1889	+0·79
1857	1·55	1868	0·77	1879	1·05	1890	1·19
1858	1·54	1869	0·91	1880	0·42	1891	1·49
1859	1·50	1870	1·08	1881	0·47	1892	1·77
1860	1·51	1871	1·01	1882	+0·13	1893	2·10
1861	1·37	1872	1·18	1883	−0·24	1894	2·04
1862	1·11	1873	1·44	1884	−0·97	1895	2·02
1863	1·31	1874	1·73	1885	−0·19	1896	2·04
1864	1·18	1875	1·90	1886	+0·29	1897	+1·55
1865	0·73	1876	1·54	1887	0·16		
1866	+0·72	1877	+1·46	1888	+0·44		

The results for the present year, so far as they go, show that the level error will be at least 0·4 smaller than in 1897.



The above values for the level error are represented in the figure, and it is evident that my conjecture of a period of about 19 years is fully borne out.

Right Ascensions and Declinations of Eight Stars of the Constellation Aquarius; also their probable Proper Motions. By C. J. Merfield.

Introduction.—The following coordinates in right ascension and declination, together with the most probable values of the proper motions, are the result of some inquiry necessary to establish good positions for certain stars of the constellation *Aquarius*, and used by Mr. J. Tebbutt as comparison stars when observing the minor planet (194) *Procne** during the year 1897. As these positions may be useful for other purposes, the writer presents the results to the Society.

The author has to acknowledge especially the kindness shown to him by Mr. H. C. Russell, C.M.G., for granting permission to consult the various star catalogues contained in the Library of the Sydney Observatory. Also his thanks are due to Mr. R. C. Walker, Principal Librarian for the Sydney Public Library, and to Mr. J. Tebbutt, of Windsor, for their kindly assistance.

The methods of Dr. H. S. Davis have been followed, and a *résumé* is given in the following paragraphs :—

Precession.—The year 1875 is the epoch of reduction selected. For this purpose the formulæ of Professor Hill and the constants of Peters and Struve have been adopted. These have been taken from the *Report on the Declinations of Stars employed in Latitude Work with the Zenith Telescope.*

Introducing these constants into the formulæ we have the following :—

$$\frac{d\alpha}{dt} = 3^s{\cdot}07225 + [0{\cdot}126115] \sin\alpha \tan\delta + \mu$$

$$\frac{d\delta}{dt} = [1{\cdot}302206] \cos\alpha + \mu'$$

$$\frac{d\mu}{dt} = [5{\cdot}9877\text{–}10]\ \mu \cos\alpha \tan\delta + [4{\cdot}8116\text{–}10]\ \mu' \sin\alpha \sec^2\delta + [4{\cdot}987\text{–}10]\ \mu\mu' \tan\delta$$

$$\frac{d^2\alpha}{dt^2} = [4{\cdot}63380\text{–}10]_n \left(\frac{d\alpha}{dt} - \mu\right) + [5{\cdot}98778\text{–}10] \left(\frac{d\alpha}{dt} + \mu\right) \cos\alpha \tan\delta + [4{\cdot}81169\text{–}10] \left(\frac{d\delta}{dt} + \mu'\right) \sin\alpha \sec^2\delta + [4{\cdot}9866\text{–}10]\ \mu\mu' \tan\delta + 0^s{\cdot}000\ 032\ 210$$

$$\frac{d^2\delta}{dt^2} = [4{\cdot}63380\text{–}10]_n \left(\frac{d\delta}{dt} - \mu'\right) + [7{\cdot}16387\text{–}10]_n \left(\frac{d\alpha}{dt} + \mu\right) \sin\alpha + [6{\cdot}7367\text{–}10]_n\ \mu^2 \sin 2\delta$$

$$\frac{d^3\delta}{dt^3} = [2{\cdot}0987\text{–}10] \left(\frac{d\alpha}{dt} + \frac{\mu}{2}\right) \sin\alpha + [7{\cdot}1638\text{–}10] \left(\frac{d^2\alpha}{dt^2} + \frac{d\mu}{dt}\right) + [3{\cdot}0255\text{–}10]_n \left(\frac{d\alpha}{dt} + \mu\right) \left(\frac{d\alpha}{dt}\right) \cos$$

* See *Astronomical Journal*, vol. xviii. No. 420.

in which α, δ, μ, μ' represent respectively the right ascension, declination, and the annual proper motions which have been assumed for the adopted epoch. The numbers in brackets are logarithms.

For convenience the following notation* has been adopted; that is, let

$$J = \frac{d\alpha}{dt} \qquad K = \frac{d^2\alpha}{dt^2} \times 10^2$$

$$L = \frac{d\delta}{dt} \qquad M = \frac{d^2\delta}{dt^2} \times 10^2 \qquad N = \times \frac{d^3\delta}{dt^3} \times \frac{1}{6} \times 10^6$$

Also let T be the epoch of any catalogue, and denote the right ascension and declination given in the catalogue by α_T, δ_T.

Then

$$\alpha_{1875} = \alpha_T + J(1875 - T) + K\frac{-(1875-T)^2}{200}$$

$$\delta_{1875} = \delta_T + L(1875 - T) + M\frac{-(1875-T)^2}{200} + N\left(\frac{1875-T}{100}\right)^3$$

Proper Motions.—The right ascensions or declinations given in the catalogues that have been reduced to the epoch of the catalogue with a proper motion μ_α or μ_δ have been corrected by the addition of the quantity

$$(T - t)\ (\mu - \mu_\alpha)$$

or

$$(T - t)\ (\mu' - \mu_\delta)$$

in which T denotes the epoch of the catalogue, and t the date of observation of the star. In several cases the value of t could not be found from the catalogues examined, nor could information be obtained from the libraries at disposal; the value of t has therefore been taken equal to T under such circumstances.

Systematic Corrections.—To reduce the resulting positions for 1875 to the system of the *Fundamental-Catalog für die Zonen-Beobachtungen der Astronomischen Gesellschaft*, the results of Professor Auwers have been employed, and interpolated from the tables published in the *Astronomische Nachrichten*, No. 3195–96 and 3413–14. In some cases the catalogues used have not been investigated by Auwers, so that a correction of 0·00 has been used.

* The notation of Dr. Davis has been adopted throughout, with some few alterations, to meet the requirements of this inquiry. See "Variation of Latitude of New York City."

Weights.—To each catalogue a weight has been assigned, according to the number of observations, and based approximately upon the relative magnitude of their respective probable errors. In cases where the number of observations was not directly or otherwise obtainable, then the assumption is made that at least one observation has been taken.

Probable Errors.—The probable errors of the several deductions have been computed from the squares of the residuals by the usual formulæ. Dr. H. S. Davis has pointed out that these formulæ put the probable errors of each star on an independent basis. On the same line and in the last column of the table of results there will be found a coefficient to multiply the probable errors given, so that they will be comparable and applicable for use in combination with the probable errors of observation of other quantities. This coefficient depends on the number of catalogues used.

Formulæ for Adjustment.—The data of each catalogue give an equation of condition of the usual form

$$\sqrt{p}\{\alpha_0 - [B + \Delta\mu_0 (t - T_0)] = R\}$$

in which

p = the weight ;
α_0 = the seconds of desired mean right ascension for the epoch T_0 ;
B = the seconds of observed right ascension after the systematic correction has been applied ;
$\Delta\mu_0$ = the correction to be *subtracted* from the assumed value of μ ;
T_0 = the mean date by weight of all the observations ;
R = the residual after substituting the derived values of the unknowns.

A similar equation for the declination is obtained by replacing α_0 by δ_0 and accenting B, $\Delta\mu_0$, and R.

The solution of these equations of condition is not so laborious if the method of Professor Safford be adopted ; thus by assuming T_0 as the initial date we have

$$T_0 = \frac{[pt]}{[p]} \text{ and } \alpha_0 = \frac{[pB]}{[p]}$$

Substituting these values in the equations of condition, and letting $\alpha_0 - B = E$, $t - T_0 = C$, and $pC = D$, then

$$\Delta\mu_0 = \frac{[DE]}{[CD]}$$

Results. Right Ascensions. Epoch 1875.

Mr. Tebbutt's Number.	Name.		Date of Observation.	Number of Observations.	Number of Catalogues.	α_{1875}		J'	K	Proper Motion μ.	Probable error of α at the epoch 1875.	Probable error of μ at the date of observation.	η
			T_0	[n]	m	h m	s	s	s	s	s	s	
11	Radcliffe	6025	1877·67	14	4	22 20	0·746	+3·1518	−0·0071	0·0000	±0·002	±0·0001	8·50
15	Radcliffe	6026	1859·60	47	12	22 20	3·216	+3·1909	−0·0093	+0·0021	±0·006	±0·0002	1·44
7	Radcliffe	6038	1877·08	12	5	22 23	56·967	+3·1242	−0·0057	+0·0052	±0·014	±0·0005	0·92
14	Radcliffe	6040	1862·78	548	40	22 24	1·845	+3·1810	−0·0088	−0·0026	±0·002	±0·0001	1·27
10	Radcliffe	6041	1869·18	20	8	22 24	44·855	+3·1404	−0·0065	+0·0076	±0·014	±0·0007	0·73
	Radcliffe	6042	1874·98	15	8	22 24	50·719	+3·1057	−0·0047	−0·0055	±0·014	±0·0006	0·73
5	Radcliffe	6073	1862·33	27	8	22 34	19·851	+3·1081	−0·0046	−0·0042	±0·017	±0·0005	0·63
6	Piazzi xxii	191	1861·98	19	8	22 35	38·343	+3·1070	−0·0045	0·0000	±0·011	±0·0004	1·06

Declinations. Epoch 1875.

Mr. Tebbutt's Number.	Name.		Date of Observation.	Number of Observations.	Number of Catalogues.	δ_{1875}	L′	M	N	Proper Motion μ'	Probable error of δ at the epoch 1875	Probable error of μ' at the date of observation.	η'
			T_0	[n]	m	° ′ ″	″	″	″	″	″	″	
11	Radcliffe	6025	1877·67	15	4	− 8 0 42·317	+ 18·1758	+ 0·1864	− 0·1693	− 0·0465	± 0·070	± 0·0045	2·51
15	Radcliffe	6026	1857·49	43	13	− 11 51 47·084	+ 18·1773	+ 0·1890	− 0·1757	+ 0·0022	± 0·099	± 0·0025	1·09
7	Radcliffe	6038	1876·71	9	5	− 5 27 30·348	+ 18·3187	+ 0·1775	− 0·1660	+ 0·0352	± 0·228	± 0·0089	0·78
14	Radcliffe	6040	1864·41	367	39	− 11 19 1·199	+ 18·3216	+ 0·1806	− 0·1747	− 0·0169	± 0·032	± 0·0014	1·09
10	Radcliffe	6041	1869·17	22	8	− 7 11 32·250	+ 18·3470	+ 0·1770	− 0·1686	− 0·1258	± 0·155	± 0·0085	0·73
3	Radcliffe	6042	1875·63	14	7	− 3 33 3·830	+ 18·3504	+ 0·1748	− 0·1631	− 0·0284	± 0·197	± 0·0079	0·67
5	Radcliffe	6073	1863·88	27	8	− 4 12 15·540	+ 18·6694	+ 0·1575	− 0·1657	− 0·0319	± 0·103	± 0·0032	1·25
6	Piazzi xxii	191	1863·46	13	6	− 4 7 34·237	+ 18·7108	+ 0·1550	− 0·1658	− 0·0059	± 0·075	± 0·0029	2·24

NOTE.

Stars No. 10, 13, 14, have been used by Professor Frisby. See *Astronomical Journal*, No. 415. Radcliffe numbers refer to the 1890 Catalogue.

$$\alpha_T = \alpha_{1875} + (J' + \mu)\ (T - 1875) + K\ \frac{(T-1875)^2}{200}$$

$$\delta_T = \delta_{1875} + (L' + \mu')\ (T - 1875) + M\ \frac{(T-1875)^2}{200} + N\left(\frac{T-1875}{100}\right)^3$$

Occultations of Ceres and of Venus, observed at the Cambridge Observatory.

(*Communicated by Professor Sir R. S. Ball, Director of the Observatory.*)

Occultation of Ceres, 1897 *November* 13.

Reappearance. First seen G.M.T. $11^h\ 31^m\ 29^s{\cdot}2$.

The observation was made with the Northumberland equatorial, aperture $11\frac{3}{4}$ in. The seeing was fair. The planet was first seen as a very unsteady and ill-defined patch of light, which grew steadily brighter, and was estimated as of full brightness 5^s after its first appearance.

Occultation of Venus, 1898 *May* 22.

Disappearance. Second contact G.M.T. $6^h\ 51^m\ 47^s{\cdot}0$.

Reappearance. Third contact G.M.T. $7^h\ 30^m\ 50^s{\cdot}1$.

The observations were made with the Northumberland equatorial. Seeing was very unsteady. First contact was missed. Bisection at disappearance was noted at $6^h\ 51^m\ 27^s$, which is uncertain by one or two seconds owing to the unsteadiness of the image. The planet lingered for several seconds as an irregular line of light, and then went out sharply, the observation of the time of second contact being noted as good. The observation of third contact was noted, "not more than half a second late, if so much." The seeing was so bad that no attempt was made to estimate the time of bisection at reappearance. The time of fourth contact was noted as $7^h\ 31^m\ 30^s$, which is uncertain by several seconds.

The observations were made by Mr. A. R. Hinks.

Cambridge Observatory:
1898 *May* 25.

Observations of Comet b 1898 (*Perrine*) *made at the Royal Observatory, Greenwich.*

(*Communicated by the Astronomer Royal.*)

The observations were made with the Sheepshanks equatorial, aperture 6·7 inches, by taking transits over two cross-wires at right angles to each other, and each inclined 45° to the parallel of declination. Magnifying power 55.

Greenwich Mean Solar Time. 1898. d h m s	Observer.	☄ − ✶ R.A. m s	Corr. for Refraction. s	Log Factor of Parallax.	☄ − ✶ N.P.D. ′ ″	Corr. for Refraction. ″	Log Factor of Parallax.	No. of Comps.	Apparent R.A. h m s	Apparent N.P.D. ° ′ ″	Comp. Star.
May 18 12 8 9	A. C.	−0 15·30	0·00	9·4644	−0 27·0	−0·1	0·9018	3	2 7 46·47	34 40 23·9	*a*

Notes.

The observations are corrected for refraction but not for parallax. They are also corrected for the error of inclination of the wires and for the motion of the comet.

The initials A. C. are those of Mr. Crommelin.

Comparison Star.

	Star's Name.	Assumed R.A. 1898·0. h m s	Assumed N.P.D. 1898·0. ° ′ ″	Authority.
a	B.D. + 55°, No. 552	2 8 1·31	34 40 59·9	Cambridge (U.S.) and Helsingfors-Gotha Astr. Gesell. Catalogues.

Royal Observatory, Greenwich:
1898 *June* 10.

Observations of Phenomena of Jupiter's Satellites with the 8-inch Equatorial of the Windsor Observatory, New South Wales, in the Year 1897. By John Tebbutt.

Day of Obs. 1897.	Satellite.	Phenomenon.	Phase.	Mag. Power.	G.M.T. of Observation. h m s	Mean Time of *N. Almanac.* h m s
Feb. 22	I.	Tr. Egr.	Bisection	70	23 57 2	24 0 0
23	I.	,,	Ext. contact	,,	0 2 7	
Mar. 8	III.	Tr. Ingr.	Ext. contact	,,	23 35 20	
8	III.	,,	Bisection	,,	23 39 14	23 40 0
8	III.	,,	Int. contact	,,	23 43 9	
25	I.	Ecl. R.	First seen	,,	23 11 59	23 12 11
25	I.	,,	Full brightness	,,	23 15 0	
27	II.	Occ. D.	First contact	,,	22 47 32	
27	II.	,,	Bisection	,,	22 51 7	22 53 0
27	II.	,,	Last seen	,,	22 53 51	
30	IV.	Occ. R.	Bisection	,,	22 54 24	22 56 0
30	IV.	,,	Last contact	,,	23 1 23	
Apr. 1	I.	Occ. D.	First contact	,,	21 55 24	
1	I.	,,	Bisection	,,	21 58 59	22 0 0
1	I.	,,	Last seen	,,	22 1 18	
5	II.	Tr. Egr.	Int. contact	,,	22 8 36	
5	II.	,,	Bisection	,,	22 12 20	22 12 0
5	II.	,,	Ext. contact	,,	22 15 45	
9	I.	,,	Int. contact	,,	23 23 6	
9	I.	,,	Bisection	,,	23 26 11	23 27 0
9	I.	,,	Ext. contact	,,	23 29 25	
16	IV.	Ecl. D.	Began to fade	,,	20 7 25	
16	IV.	,,	Last seen	,,	20 15 46	20 15 27
16	I.	Tr. Ingr.	Ext. contact	,,	22 54 57	
16	I.	,,	Bisection	,,	22 56 51	22 56 0
16	I.	,,	Int. contact	,,	22 59 6	
17	I.	Occ. D.	First contact	,,	19 57 50	
17	I.	,,	Bisection	,,	20 1 0	20 4 0
17	I.	,,	Last seen	,,	20 3 59	
17	I.	Ecl. R.	First seen	,,	23 24 45	23 24 58
17	I.	,,	Full brightness	,,	23 28 35	

Day of Obs. 1897.	Satellite.	Phenomenon.	Phase.	Mag. Power.	G.M.T. of Observation. h m s	Mean Time of *N. Almanac*, h m s
Apr. 26	I.	Ecl. R.	First seen	70	19 48 23	19 48 34
26	I.	„	Full brightness	„	19 50 54	
28	II.	Occ. D.	First contact	138	21 40 8	
28	II.	„	Bisection	„	21 43 23	21 43 0
28	II.	„	Last seen	„	21 45 28	
May 1	III.	Ecl. R.	First seen	„	22 15 36	22 16 2
1	I.	Occ. D.	First contact	131	23 43 19	
1	I.	„	Bisection	„	23 45 58	23 46 0
1	I.	„	Last seen	„	23 47 38	
2	I.	Tr. Ingr.	Ext. contact	„	21 6 39	
2	I.	„	Bisection	„	21 9 4	21 6 0
2	I.	„	Int. contact	„	21 11 19	
2	I.	Tr. Egr.	Int. contact	„	23 21 37	
2	I.	„	Bisection	„	23 24 6	23 25 0
2	I.	„	Ext. contact	„	23 27 1	
2	Shd. I.	Transit	On cent. merid.	„	23 16 43	
3	I.	Ecl. R.	First seen	175	21 43 31	21 43 34
8	III.	Occ. R.	Last contact	132	21 18 56	21 22 0
8	III.	Ecl. D.	Last seen	74	22 59 33	22 56 0
10	I.	Occ. D.	First contact	168	20 6 8	
10	I.	„	Bisection	„	20 8 28	20 7 0
10	I.	„	Last seen	„	20 9 38	
14	II.	Tr. Ingr.	Ext. contact	„	20 52 35	
14	II.	„	Bisection	„	20 55 30	20 56 0
14	II.	„	Int. contact	„	20 58 44	
15	III.	Occ. D.	First contact	132	21 37 52	
15	III.	„	Bisection	„	21 41 16	21 40 0
15	III.	„	Last seen	„	21 44 40	
16	II.	Ecl. R.	First seen	74	21 22 20	21 22 2
16	II.	„	Full brightness	„	21 26 36	
17	I.	Occ. D.	First contact	168	21 58 14	
17	I.	„	Bisection	„	22 1 4	22 1 0
17	I.	„	Last seen	„	22 2 33	
25	I.	Tr. Ingr.	Ext. contact	„	21 16 11	
25	I.	„	Bisection	„	21 18 16	21 16 0
25	I.	„	Int. contact	„	21 20 45	
26	I.	Ecl. R.	First seen	74	21 57 40	21 57 44

Day of Obs. 1897.	Satellite.	Phenomenon.	Phase.	Mag. Power.	G.M.T. of Observation. h m s	Mean Time of *N. Almanac.* h m s
June 26	I.	Tr. Egr.	Int. contact	168	20 16 9	
26	I.	„	Bisection	„	20 20 3	20 22 0
26	I.	„	Ext. contact	„	20 22 53	
July 1	II.	Occ. D.	First contact	„	21 14 28	
1	II.	„	Bisection	„	21 17 3	21 16 0
1	II.	„	Last seen	„	21 19 42	
4	I.	Ecl. R.	First seen	74	20 31 19	20 31 1
19	II.	„	First seen	„	20 33 16	20 32 44
19	II.	„	Full brightness	„	20 35 52	

Notes.

February 22–23.—Definition pretty good.

March 8.—Good definition.

March 25.—Sky beautifully clear, and images steady and well defined.

March 27.—Good definition.

March 30.—Images steady and fairly well defined.

April 1.—Good definition.

April 5.—Definition pretty good.

April 9.—Definition pretty good, but first phase unsatisfactory.

April 16.—The time of final disappearance doubtful to a second or two in consequence of the very slow fading of the satellite; the definition was good. The definition was also good at the ingress. Clouds prevented an observation of the reappearance from eclipse.

April 17.—Images very tremulous and badly defined, particularly at the last phase of the occultation. Definition pretty good at the eclipse, but full Moon present. In observing the first phase of the eclipse the planet was hidden by a bar of the micrometer eyepiece.

April 26.—Beautifully clear and definition good, but the twilight had scarcely disappeared. The first phase was observed as on the 17th. The square bar micrometer, which was in adjustment for the observation of Comet I., 1897 (Perrine), was employed from February 22 to April 26.

April 28.—Good definition.

May 1.—Definition good for both phenomena, except at the last phase of the occultation. Sky beautifully clear.

May 2.—Definition pretty good for the ingress and good for the egress. The transit of the shadow was observed by estimation only.

May 3.—Good definition.

May 8.—Definition pretty good for the occultation phase. For the eclipse the sky was beautifully clear and the definition good, but the Moon was present.

May 10.—Definition pretty good, but the second phase was observed late.

May 14.—Definition bad and images tremulous.

May 15.—Images pretty steady and well defined.

May 16.—Definition pretty good, but images rather tremulous, and the full Moon present.

May 17.—Definition good at first two phases, but bad at the last phase.

May 25.—Definition bad and images tremulous.

May 26.—Light clouds occasionally passing over the planet, but at the critical time it shone clear and bright.

June 26.—Definition pretty good.

July 1.—Bad definition; last phase extremely difficult to observe.
July 4.—Sky beautifully clear and Moon not far from planet. The images were greatly blurred and the observation was very unsatisfactory.
July 19.—Sky beautifully clear, but the images were unsteady and badly defined.

Except where otherwise stated, an occulting bar was not employed in the eclipse observations. The times given are the Windsor mean times of observation diminished by 10 hrs. 3 mins. 20·51 secs., and extend to the nearest second. The observations of full brightness in the eclipses are at the best only rough approximations. In determining these times the increasing light of the satellite was repeatedly compared with the other visible satellites.

Private Observatory, Windsor, N.S. Wales:
1898 *April* 18.

Ephemeris for Physical Observations of

Greenwich Noon.	P.	L−O.	B.	A−L.	*B.*	Q.	E.	Light Time.
1898.	°	°	°	°	°	°	°	m
June 27	321·16	143·13	−14·98	−35·17	−24·13	252·22	34·29	14·96
29	321·20	144·51	14·45	35·22	23·96	252·67	34·49	14·89
July 1	321·26	145·88	13·92	35·26	23·78	253·13	34·70	14·83
3	321·34	147·25	13·39	35·31	23·59	253·59	34·91	14·76
5	321·45	148·60	12·85	35·35	23·40	254·05	35·12	14·69
7	321·58	149·94	−12·31	−35·39	−23·19	254·51	35·32	14·61
9	321·74	151·28	11·76	35·44	22·97	254·98	35·53	14·54
11	321·92	152·60	11·21	35·47	22·74	255·47	35·73	14·47
13	322·13	153·92	10·66	35·50	22·50	255·96	35·93	14·40
15	322·36	155·23	10·10	35·53	22·25	256·46	36·12	14·33
17	322·61	156·53	−9·54	−35·56	−21·99	256·97	36·31	14·25
19	322·88	157·81	8·98	35·59	21·72	257·49	36·50	14·18
21	323·16	159·08	8·41	35·61	21·44	258·01	36·70	14·11
23	323·46	160·35	7·84	35·64	21·16	258·54	36·89	14·03
25	323·79	161·61	7·27	35·67	20·87	259·08	37·08	13·96
27	324·14	162·86	−6·70	−35·71	−20·57	259·62	37·26	13·88
29	324·51	164·10	6·14	35·74	20·26	260·16	37·43	13·80
31	324·90	165·33	5·58	35·77	19·94	260·71	37·61	13·71
Aug. 2	325·31	166·56	5·01	35·80	19·62	261·26	37·78	13·63
4	325·73	167·78	4·44	35·83	19·29	261·82	37·95	13·55
6	326·17	168·98	−3·87	−35·86	−18·96	262·38	38·13	13·47
8	326·63	170·18	3·31	35·89	18·62	262·94	38·29	13·39
10	327·10	171·38	2·75	35·92	18·27	263·50	38·45	13·30
12	327·58	172·57	2·19	35·95	17·91	264·06	38·61	13·21
14	328·08	173·76	1·63	35·99	17·55	264·62	38·76	13·12
16	328·60	174·94	−1·07	−36·03	−17·19	265·18	38·91	13·03
18	329·12	176·11	−0·52	36·07	16·82	265·74	39·06	12·94
20	329·65	177·27	+0·03	36·11	16·45	266·30	39·20	12·85
22	330·20	178·42	0·57	36·15	16·07	266·86	39·34	12·76
24	330·76	179·56	1·11	36·18	15·69	267·42	39·47	12·66
26	331·32	180·69	1·64	36·21	15·30	267·98	39·60	12·57
28	331·89	181·82	2·17	36·25	14·91	268·53	39·72	12·47
30	332·47	182·94	+2·70	−36·29	−14·51	269·08	39·84	12·38

Mars, 1898. By A. C. D. Crommelin.

Greenwich Noon.		Appar. Diam.	Defect of Illumination. q	Central Meridian. ω_o	Passage of Zero Meridian.			
1898.		"	"	°	h	m	h	m
June	27	5·33	0·46	4·53	...	...	23	41
	29	5·35	0·46	344·95	1	2	0	22
July	1	5·37	0·47	325·38	2	22	1	42
	3	5·39	0·48	305·82	3	42	3	2
	5	5·42	0·49	286·27	5	3	4	23
	7	5·46	0·50	266·73	6	23	5	43
	9	5·49	0·51	247·20	7	43	7	3
	11	5·52	0·51	227·67	9	4	8	23
	13	5·55	0·52	208·15	10	24	9	44
	15	5·58	0·53	188·64	11	44	11	4
	17	5·61	0·54	169·14	13	5	12	25
	19	5·65	0·55	149·65	14	25	13	45
	21	5·68	0·56	130·17	15	45	15	5
	23	5·71	0·57	110·71	17	5	16	25
	25	5·74	0·58	91·26	18	25	17	45
	27	5·77	0·59	71·82	19	45	19	5
	29	5·80	0·59	52·39	21	5	20	25
	31	5·83	0·60	32·97	22	24	21	45
Aug.	2	5·87	0·61	13·55	23	44	23	4
	4	5·91	0·62	354·13	0	24	...	...
	6	5·95	0·63	334·72	1	44	1	4
	8	5·98	0·64	315·32	3	4	2	24
	10	6·02	0·65	295·93	4	23	3	44
	12	6·06	0·66	276·54	5	43	5	3
	14	6·10	0·67	257·16	7	2	6	22
	16	6·14	0·68	237·79	8	22	7	42
	18	6·18	0·69	218·43	9	41	9	2
	20	6·22	0·70	199·08	11	1	10	21
	22	6·27	0·71	179·74	12	21	11	41
	24	6·31	0·72	160·40	13	40	13	0
	26	6·36	0·73	141·07	14	59	14	20
	28	6·40	0·74	121·75	16	19	15	39
	30	6·45	0·75	102·44	17	38	16	58
							0	0

Greenwich Noon.	P.	L−O.	B.	A−L.	*B.*	Q.	E.	Light Time.
1898.	°	°	°	°	°	°	°	m
Sept. 1	333·06	184 06	+ 3·22	−36·33	−14·11	269·62	39·96	12·28
3	333·66	185·17	3·73	36·37	13·71	270·16	40·07	12·18
5	334·26	186·27	4·24	36·41	13·30	270·70	40·18	12·08
7	334·87	187·37	4·75	36·46	12·89	271·24	40·28	11·98
9	335·49	188·47	+ 5·25	−36·52	−12·48	271·77	40·37	11·88
11	336·11	189·55	5·73	36·56	12·07	272·30	40·46	11·78
13	336·73	190·63	6·20	36·61	11·65	272·81	40·54	11·67
15	337·36	191·70	6·67	36·65	11·23	273·32	40·61	11·56
17	337·99	192·76	7·13	36·69	10·81	273·82	40·67	11·46
19	338·63	193·81	+ 7·59	−36·73	−10·39	274·31	40·73	11·35
21	339·27	194·86	8·03	36·78	9·97	274·80	40·78	11·24
23	339·90	195·90	8·47	36·82	9·55	275·28	40·82	11·13
25	340·52	196·93	8·90	36·85	9·12	275·75	40·85	11·02
27	341·15	197·94	9·32	36·87	8·69	276·21	40·88	10·91
29	341·78	198·94	+ 9·73	−36·89	− 8·26	276·65	40·89	10·80
Oct. 1	342·40	199·94	10·13	36·91	7·83	277·08	40·89	10·68
3	343·02	200·93	10·52	36·93	7·39	277·50	40·88	10·57
5	343·63	201·91	10·91	36 94	6·96	277·92	40·86	10·45
7	344·23	202·87	11·28	36·94	6·53	278·33	40·83	10·33
9	344·84	203·82	+ 11·64	−36·93	− 6·10	278·73	40·79	10·21
11	345·45	204·77	12·00	36·92	5·67	279·12	40·74	10·09
13	346·05	205·70	12·34	36·90	5·23	279·49	40·68	9·97
15	346·64	206·61	12·67	36·87	4·80	279·85	40·59	9·85
17	347·22	207·51	12·98	36 83	4·37	280·19	40·49	9·72
19	347·79	208·40	+ 13·28	−36·79	− 3·93	280·52	40·38	9·60
21	348·35	209·27	13·58	36·73	3·50	280·84	40·25	9·48
23	348·90	210·12	13·86	36·66	3·06	281·15	40·10	9·35
25	349·45	210·96	14·13	36·57	2·63	281·44	39·93	9·23
27	349·98	211·78	14·38	36·47	2·20	281·71	39·74	9·11
29	350·49	212·56	+ 14·62	−36·33	− 1·77	281·97	39·53	8·99
31	350·98	213·31	14·85	36·17	1·35	282·21	39·30	8·86
Nov. 2	351·46	214·04	15·07	35·99	0·93	282·45	39·04	8·73
4	351·92	214·74	15·28	35·79	0·50	282.67	38·77	8·61
6	352·35	215·42	15·44	35·57	0·07	282·87	38·48	8·48
8	352·76	216 06	+ 15·60	−35·31	+ 0·35	283·06	38·16	8·35
10	353·14	216·67	+ 15·74	−35·03	+ 0·77	283·24	37·81	8·23

Greenwich Noon. 1898.		Appar. Diam.	Defect of Illumination. q	Central Meridian. ω_0	Passage of Zero Meridian.			
		″	″	°	h	m	h	m
Sept.	1	6·50	0·76	83·13	18	58	18	18
	3	6·56	0·77	63·83	20	17	19	38
	5	6·61	0·78	44·53	21	37	20	57
	7	6·66	0·79	25·24	22	57	22	17
	9	6·72	0·80	5·95	...	...	23	36
	11	6·78	0·81	346·67	0	55	0	16
	13	6·84	0·82	327·40	2	14	1	35
	15	6·90	0·83	308·14	3	33	2	54
	17	6·97	0·84	288·89	4	52	4	13
	19	7·04	0·85	269·65	6	11	5	31
	21	7·12	0·86	250·42	7	30	6	50
	23	7·20	0·87	231·19	8	49	8	9
	25	7·27	0·89	211·97	10	8	9	28
	27	7·34	0·90	192·76	11	27	10	47
	29	7·41	0·91	173·56	12	45	12	6
Oct.	1	7·50	0·92	154·37	14	5	13	25
	3	7·57	0·93	135·20	15	23	14	44
	5	7·64	0·93	116·04	16	42	16	2
	7	7·72	0·94	96·89	18	0	17	21
	9	7·81	0·95	77·75	19	19	18	40
	11	7·91	0·96	58·62	20	38	19	59
	13	8·02	0·97	39·51	21	57	21	18
	15	8·12	0·97	20·41	23	16	22	36
	17	8·23	0·98	1·32	...	...	23	55
	19	8·33	0·99	342·24	1	13	0	34
	21	8·43	1·00	323·18	2	31	1	52
	23	8·54	1·00	304·14	3	49	3	10
	25	8·65	1·01	285·13	5	7	4	28
	27	8·77	1·01	266·15	6	25	5	46
	29	8·89	1·01	247·18	7	43	7	4
	31	9·02	1·02	228·23	9	1	8	22
Nov.	2	9·15	1·02	209·31	10	19	9	40
	4	9·29	1·02	190·42	11	37	10	58
	6	9·42	1·02	171·56	12	55	12	16
	8	9·56	1·02	152·73	14	12	13	33
	10	9·70	1·02	133·94	15	29	14	51

The ephemeris has been constructed with the same constants as those employed by Mr. Marth in his ephemeris for the last opposition (*Monthly Notices*, lvi. 7, p. 394). The position of the north pole of *Mars* for 1898·0 is taken as R.A. 317°·15, N.P.D. 37°·43, this being the value deduced by Struve from observations of the satellites : it differs considerably from values deduced from observations of markings on the planet. The following are the three most recent determinations by this method, all reduced to 1898·0 :—

	R.A.	N.P.D.
Schiaparelli	318°·27	36°·31
Lohse, 1883	318°·34	36°·63
Lohse, 1893	318°·45	36°·16

Mean value R.A. 318°·35, N.P.D. 36°·37.

I shall be glad of the opinions of observers as to the advisability of continuing to use Struve's value, or substituting the mean of the other three.

P denotes the position-angle of the axis of *Mars*, L − O + 180° the longitude of the Earth referred to the plane of *Mars*' equator, and reckoned from O, the point of the vernal equinox of *Mars*' northern hemisphere, Λ − O + 180° the corresponding quantity for the Sun, B, *B* the latitudes of the Earth and Sun reckoned from the plane of *Mars*' equator, Q the position-angle of the greatest defect of illumination, *q* the amount of the greatest defect of illumination, E the areocentric angle between Earth and Sun.

The diameter of the planet at distance unity is assumed (as before) to be 9″·60, that used in the *Nautical Almanac* being 9″·36.

The zero meridian and period of rotation have been carried on unchanged from Mr. Marth's last ephemeris, the assumed sidereal rotation in 24 hours being 350°·89202.

The times of passage of the zero meridian are given approximately. Where greater accuracy is required, the time may be deduced by interpolating between two successive tabulated values of the central meridian at noon.

The winter solstice of *Mars*' northern hemisphere occurred 1898 May 31ᵈ·7, and the spring equinox occurs 1898 November 6ᵈ·3.

Benvenue, Ulundi Road, Blackheath, S.E.:
1898 *June* 20.

Erratum in Mr. Anderson's paper, Monthly Notices, vol. lviii. p. 380, note to observation of March 8 :—

For ζ *Tauri* read ξ *Tauri*.